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JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION

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INDEX NUMBERS AND PUBLIC UTILITY VALUATION

By Robert W. Harbeson
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An important development in the public utility valuation controversy is the recent decision of the United States Supreme Court in *West et al.* vs. *Chesapeake and Potomac Telephone Company of Baltimore City*,[1] handed down on June 3, 1935, in which index numbers as used by the Public Service Commission of Maryland were declared to be "inappropriate for obtaining the value of a going telephone plant"[2] and a violation of the principle of due process. It is the writer's opinion that on the whole the decision was proper in view of the facts of this particular case, but that if in subsequent litigation it appears that the Court intends this decision to stand as a general condemnation of valuations based upon index numbers it would be an undesirable extension of the scope of judicial review and a barrier to the economical and effective administration of the law governing valuation for rate-making purposes as developed in previous decisions of the Court. Some competent students, who apparently interpret the decision in the latter way, hold that if it is allowed to stand unmodified it may even lead to the complete breakdown of regulation. This situation would be welcomed both by those who desire government ownership of public utilities, and by those who believe that it would lead to the establishment of the administratively practicable prudent investment basis of valuation.

I

The facts of this case may be briefly reviewed. On November 28, 1933, after extended hearings, the Public Service Commission of Maryland ordered the Chesapeake and Potomac Telephone Company to

[1] 55 Sup. Ct. 894.
[2] *Ibid.*, p. 896.

put into effect on January 1, 1934, reductions in rates which would diminish its annual net income by an estimated $1,000,000. The Commission had taken the Company's valuation as determined by the District Court in 1923 (about $6,000,000 in excess of the Company's book value as it then stood) plus the net additions after retirements each year, and had translated these by means of index numbers into an equivalent dollar value as of December 31, 1932. The valuation at which it arrived as of the latter date was $32,610,327, representing the weighted average of calculations based on sixteen price indices as shown in Table I.[3] On the basis of an estimated net revenue for 1934

TABLE I

SUMMARY OF INDEXES USED, WEIGHTS ASSIGNED, AND RESULTING VALUES IN THE BALTIMORE TELEPHONE CASE*

Identification of Price Trend	Weights	Depreciated Value
U. S. Department of Labor, All Commodities	4	$30,458,180
National Assn. of Purchasing Agents-Commodities	2	25,410,849
Dr. Irving Fisher's Trend-Commodities	1	28,681,140
Dun's Trend-Commodities	1	33,581,993
Bradstreets Trend-Commodities	1	24,983,624
Average of Five Commodity Trends		28,623,157
Engineering News Record—Construction	2	35,009,281
American Appraisal Co.—Construction	2	32,623,549
Richey (Old Series)—Construction	2	35,783,737
Consolidated Engineering Co.—Construction	2	34,092,828
Turner Construction Co.—Construction	1.5	33,153,303
Average of Five Construction Trends		34,552,838
U. S. Department of Labor-Building Materials	2	33,132,540
National Assn. of Purchasing Agents Bldg. Materials	1	29,965,050
Average of Two Building-Materials Trends		31,552,838
Federal Reserve Bank of New York—General	2	36,056,408
Charles W. Smith—Baltimore Wages	1.5	30,918,858
United States Department of Labor, Building Materials and Baltimore Wages	0	32,369,324
Interstate Commerce Commission, Railroad Telephone and Telegraph Lines	3	32,024,463
Company Exhibit 35, Western Electric Co. prices. (Less increase since 1929.)	3	34,567,634
Composite Trend by Accounts	0	31,108,325
Maryland Commission's Fair Value Trend		$32,610,327

* E. W. Morehouse, "Price Trends in the Baltimore Telephone Case," *Journal of Land and Public Utility Economics*, XI, 318 (August, 1935).

of $3,353,793 and a return of 6 per cent on the foregoing valuation ($1,957,271) there would remain net revenue of $1,396,522. With the rate reduction referred to above there would remain $396,522 as a "cushion" against rising prices. The Commission accepted the Company's figures for current expense, except that it reduced the annual allowance for depreciation from $2,173,000 to $1,720,724.[4] The Company had also insisted on a 7½ per cent return in place of the 6 per cent return allowed by the Commission.

[3] This is the figure for the valuation, without working capital, as of Dec. 31, 1932; the "average value of rate base for 1933" as given on p. 897 of the decision was $32,621,190. The allowance for working capital was $660,863.

[4] This is the figure given on p. 895 of the report; that given on p. 896 is $1,352,284.

Upon appeal to the District Court the Commission's order was enjoined. The lower court held that the price indices used by the Commission were inappropriate for determining present value and discarded them. While, as the Supreme Court said, the lower court purported to consider both book cost and reproduction cost, it actually fixed a valuation equal to the book cost as of December 31, 1933, less the depreciation reserve and plus $1,000,000 for working capital—a total of $39,541,921. The District Court also raised the annual depreciation allowance to $2,000,000. On this basis the net return under the Commission's order would be $1,742,005, or at the rate of 4½ per cent on the Court's valuation. The Court held that a return of less than 6 per cent was confiscatory. It will be recalled that the Commission had allowed this return on the basis of the valuation and depreciation figures determined by it.

The Supreme Court expressed dissatisfaction with the methods pursued both by the lower Court and by the Commission. The method of the former was summarily disapproved because the valuation fixed was merely the book cost shown by the company less the depreciation reserve plus an allowance for working capital. "This rough and ready approximation of value," said the Court, "is as arbitrary as that of the Commission, for it is unsupported by findings based upon evidence."[5] It stated further that it was not its function nor that of the lower court to do the work of the Commission by determining a rate base upon correct principles. Nevertheless, although the method pursued by the District Court was thus condemned, its action in enjoining the Commission's rate order was upheld.

The grounds upon which a majority of the Supreme Court rejected the Commission's methods were apparently four in number, as follows:

1. "An obvious objection is that the indices were not prepared as an aid to the appraisal of property. They were intended merely to indicate price trends."[6] Reference was made in this connection to the Interstate Commerce Commission index, which was one of those used by the Maryland Commission, and which, it was pointed out, was not intended to be used "in the determination of unit reproduction costs" upon individual properties.[7] The Court indicated, however, that price trends might properly be considered along with all other relevant factors.

2. The "wide variation of results of the employment of different indices, already mentioned, impugns their accuracy as implements of

[5] 55 Sup. Ct. 894, *op. cit.*, at p. 901.
[6] *Ibid.*, pp. 896–97.
[7] *Idem*, p. 897.

appraisal."[8] The Court called attention to the 48 per cent spread between the highest and lowest of the indices used in finding the valuation.

3. The Commission weighted the several indices "upon a principle known only to itself, and thus rendered its process of valuation even more dubious and obscure."[9] The weight given to the index of Western Electric prices was especially criticized.

4. "A more fundamental defect in the Commission's method is that the result is affected by sudden shifts in price level. . . . A public service corporation ought not, therefore, in a rate proceeding, to be permitted to claim to the last dollar an increased value consequent upon a sudden and precipitate rise in spot prices of material or labor. No more ought the value attributable to its property to be depressed by a similar sudden decline in the price level."[10]

The majority found it necessary to consider the following pronouncement in the Los Angeles Gas and Electric Case:

> The legislative discretion implied in the rate-making power necessarily extends to the entire legislative process, embracing the method used in reaching the legislative determination as well as that determination itself. We are not concerned with either, so long as constitutional limitations are not transgressed. When the legislative method is disclosed, it may have a definite bearing upon the validity of the result reached, but the judicial function does not go beyond the decision of the constitutional question. That question is whether the rates as fixed are confiscatory.[11]

The Court pointed out that this language had been used with respect to the claim that various elements of value had been ignored; that it was found, however, that though there may have been errors committed in this way they were neutralized by other allowances; and that nothing in this case justified the claim that the Court had departed from the principle announced in earlier decisions "as to the value upon which a utility is entitled to earn a reasonable return or the character of evidence relevant to that issue."[12]

Finally, the majority quoted with approval the following from *Northern Pacific Railway Company* vs. *Department of Public Works of State of Washington:*

> But where rates found by a regulatory body to be compensatory are attacked as being confiscatory, courts may inquire into the method by which its conclusion was reached. An order based upon a finding made without evidence, (case cited), *or upon a finding made upon evidence which clearly does not support it,* (case cited), is an arbitrary act against which courts afford relief. The error under discussion was of this character. It was a denial of due process.[13]

[8] *Idem*, p. 897.
[9] *Idem*, p. 897.
[10] *Ibid.*, p. 898.
[11] *Los Angeles Gas and Electric Co.* vs. *Railroad Commission*, 289. U. S. 287, at p. 304 (1933).
[12] 55 Sup. Ct. 894, *op. cit.*, p. 899.
[13] 268 U. S. 39, p. 43 (1925). Italics mine.

Mr. Justice Stone wrote a minority opinion in which Justices Brandeis and Cardozo joined. The minority pointed out, first, that the sole issue is that of confiscation and that the Court did not pass upon that issue since it rejected the valuation fixed by the District Court. But since the decree of the lower court was nevertheless upheld the minority argued that the Commission's order was set aside upon a ground not raised upon the record or considered by the lower court, and that it was set aside because of being deemed a denial of due process in the procedural sense. The minority, however, could not see that any constitutional guarantees had been violated, saying:

> The sole transgression, for which its painstaking work is set at naught, is that, in the exercise of the administrative judgment of this body "informed by experience" and "appointed by law" to deal with the very problem now presented, (case cited), it has relied upon a study of the historical cost and ascertained value of appellee's plant in the light of price indices, showing declines in prices, in arriving at the present fair value of the property, a procedure on which this Court has hitherto set the seal of its approval.[14] (citing cases).

After expressing "grave doubt" as to whether the 4½ per cent rate of return, which the rates ordered by the Commission would yield on the District Court's valuation, was confiscatory, Justice Stone proceeded to defend the use of index numbers in place of an appraisal in the present case by pointing out that an appraisal would have taken at least two years and would have cost the Company not less than $300,000 and the State a very substantial sum. The Commission's weighting of the various component indices was also defended, it being pointed out that those indices which the Commission had reason to believe were more accurate and also those which were more directly applicable to telephone property were given the greatest weights. In answer to the majority's objection that the wide variation in the indices used impugned their accuracy as implements of appraisal attention was called to the fact that the results of the highest and lowest of the indices differed from the Commission average only by 10.6 per cent and 23.4 per cent respectively; that eleven of the sixteen indices separately considered gave results ranging between $30,000,000 and $34,600,000 as compared with the Commission average of $32,610,327; and that in the New York Telephone Case, in which actual appraisals were used, the highest appraisal exceeded the lowest by 67.1 per cent. Justice Stone's further comments in defense of the use of index numbers in place of appraisals are of interest:

> In assuming the task of determining judicially the present fair replacement value of the vast properties of public utilities, courts have been projected into the most speculative undertaking imposed upon them in the entire history of English jurisprudence. . . . When we arrive at a theoretical

[14] 55 Sup. Ct. 894, *op. cit.*, p. 900.

> value based upon such uncertain and fugitive data we gain at best only an illusory certainty. No court can evolve from its inner consciousness the answer to the question whether the illusion of certainty will invariably be better supported by a study of the actual cost of the property adjusted to price trends, or by a study of the estimates of engineers based upon data which never have existed and never will. The value of such a study is a question of fact in each case, to be ascertained like any other in the light of the record, and with some regard to the expert knowledge and experience of the commission, which, in the present case, are obviously great.
>
> . . . . . .
>
> So far as the results of the use of standard price indices is impugned by their variation, an examination of the present record will disclose that the results obtained by the application of price indices to the historical cost of plant are far less variable than engineers' valuations and in general are probably more trustworthy. . . . Present fair value at best is but an estimate. Historical cost appropriately adjusted by reasonable recognition of price trends appears to be quite as common sense a method of arriving at a present theoretical value as any other.[15]

By way of answer to the passage in the Northern Pacific Case[16] quoted with approval by the majority, Justice Stone quoted another passage from the same case, as follows:

> The mere admission by an administrative tribunal of matter which under the rules of evidence applicable to judicial proceedings would be deemed incompetent (case cited), or mere error in reasoning upon evidence introduced, does not invalidate an order.[17]

Lastly, Justice Stone also quoted the passage from the Los Angeles Gas and Electric Case[18] which had been quoted by the majority, holding that the principle there announced should have been controlling in the case in question.

II

In evaluating the respective merits of the foregoing majority and minority opinions it is necessary to begin with an analysis of the composite index used by the Commission. It is a fundamental principle recognized by economists and statisticians that procedure in constructing an index number (as regards such matters as choice of items, weighting, etc.) must be determined by reference to the *purpose* for which the index number is to be used, and that an index number constructed for one purpose is not valid or reliable if used for another.[19] The Commission's composite index presumably would be regarded by economists as an index of the general price level. Such an index is recognized as being of value as an approximate measure of changes in the general purchasing power of money. But it has been increasingly

[15] 55 U. S. 894, *op. cit.*, pp. 905–6.
[16] *Supra*, p. 248.
[17] 268 U. S. 39, *op. cit.*, at p. 44. (1925).
[18] *Supra*, p. 248.
[19] For example: "The choice of methods to be employed in making an index number should be guided by the purpose for which the results are to be used. These purposes are so numerous and so diverse that it is impossible to make a single series well adapted to them all." W. C. Mitchell, *The Making and Using of Index Numbers*, Bulletin 284, Bureau of Labor Statistics, p. 112.

recognized in recent years that an index of this sort obscures many diverse and significant movements in particular groups of prices, and that for many purposes it is these movements and their interrelationships which are significant.

It follows that a composite price index suitable for measuring changes in the general purchasing power of money could not properly be used to measure changes in the cost of reproducing a telephone property. The latter purpose requires an index based upon prices actually paid by telephone companies. Hence the writer believes that the majority were on sound ground both economically and legally in rejecting the valuation based upon the Commission's composite index. For what the Commission's figure really represented was not the cost of reproducing telephone property at all, but rather a revaluing of the original investment[20] according to changes in the general purchasing power of money between 1923 and 1932. In other words, in the language of economics, what the Commission computed was the Company's "real" investment in terms of 1932 dollars. This distinction would not exist if there were complete uniformity in price movements. But as matters actually stand the diversity of movement of individual prices and groups of prices is perhaps the central fact which appears from a study of the price structure.

Likewise it is the writer's understanding that the purchasing power of money in general is not one of the "values or elements of value" which according to previous decisions of the Court must be "considered" or "given weight" in arriving at value for rate-making purposes. To be sure, there have been numerous references to past and prospective movements of the price level, as, for example, in the O'Fallon[21] and Indianapolis Water[22] cases, but these references have generally borne on the question of the relative weight to be given to original and reproduction costs. Certainly, so far as the writer is aware, the purchasing power of money in general has not taken its place alongside original cost, reproduction cost, and other specific elements of value as a component in final valuations. In the writer's opinion the principal if not the only reason for the Court's action in this case may have been an objection to a valuation dominated by a factor which has not been recognized as an element of value, rather than an objection to the use of index numbers as a short cut in place of appraisals. Hence the Court's statement that an obvious objection

[20] More precisely, it was not the original investment which was revalued in this case but, as already indicated, the original investment increased by about $6,000,000. However, this latter increase had been arrived at by the application of the Bureau of Labor Statistics wholesale price index to book cost. See *supra*, p. 247.

[21] *St. Louis and O'Fallon R. R. Co. et al.* vs. *U. S.*, 279, U. S. 461 (1929).

[22] *McCardle* vs. *Indianapolis Water Co.*, 272 U. S. 400 (1926).

to the indices was that they "were not prepared as an aid to the appraisal of property," but were "intended merely to indicate price trends."[23]

The minority imply that the majority opinion represents an improper extension of the scope of judicial review, in that in effect the Court substituted its judgment for that of the Commission on a straight question of fact—the wisdom and expediency of the weights given by the Commission to the components of its composite price index. Likewise, some writers apparently make the decision hinge largely on the majority's pronouncement that the Commission weighted the several indices "upon a principle known only to itself, and thus rendered its process of valuation even more dubious and obscure."[24] Dr. E. W. Morehouse has recently suggested what he regards as a way by which the Commission might have avoided reversal, as follows:

> The point might be made that the weighting was merely a convenient tool used in exercising judgment as to value. It was not essential to that exercise of judgment. One wonders what the Court's opinion of the value judgment would have been if the separately trended values, stated without weights, had been used and the final value judgment expressed thereon. Would the Commission's judgment figure then have been impeached merely because trends had been examined in reaching a conclusion? Would the method in that event have been found faulty, and hence the result unconstitutional? . . . It is not clearly shown how the weighting process has impaired the Commission's judgment value.[25]

Commenting on the foregoing passage *Public Utilities Fortnightly* remarks editorially as follows:

> After all, the majority opinion in the Maryland telephone case did not condemn the use of commodity indices by commissions but merely the erection of a mathematical rule of thumb that might seem to preclude the free play of individual judgment. Perhaps in the future, cases developed along this line that place emphasis upon the employment of judgment by the commissions in reaching their conclusions, may avoid the obstacle to such procedure set up by the Supreme Court in the Maryland telephone case. As it was, the Maryland commission succeeded in convincing three justices that its method was not necessarily confiscatory. Dr. Morehouse's suggestion might plausibly provide the margin of victory.[26]

While the foregoing suggestion is of unquestioned merit, in the opinion of the writer it does not touch the fundamental reason for the reversal. It was not a mere question of the right of the Commission to exercise its judgment as to using index numbers in place of an appraisal, or in the choice of, or weight to be given to, any index number *designed to measure reproduction cost or any other legally recognized element of value.* If such were the case the writer does not believe that

[23] *Supra*, p. 247.
[24] *Supra*, p. 248.
[25] E. W. Morehouse, "Price Trends in the Baltimore Telephone Case," *Journal of Land and Public Utility Economics*, XI, 318–19 (August, 1935).
[26] "Liberal Economists Deplore Maryland Telephone Decision," *Public Utilities Fortnightly* 446–49, 16, at pp. 448–49 (Sept. 26, 1935).

the majority opinion could be justified. The Commission, however, was overruled not because the foregoing questions were raised but because it avowedly and specifically was concerned with computing an index which would measure the Company's investment in terms of 1932 general purchasing power rather than an index of the cost of reproduction of telephone property, thus, to repeat, arriving at a valuation dominated by a factor which has not been legally recognized as an element in "fair value." This amounts, in the writer's opinion, to a mistake of law and a finding of fair value "upon evidence which clearly does not support it,"[27] and is an arbitrary act against which the Court properly afforded relief. The other objections raised by the majority, standing alone, would not likely, in the writer's opinion, have led to a reversal.

The Commission pointed out in its decision that the index numbers of the cost of telephone property submitted to it were, on the 1932 base, higher as of December 31, 1932, than they were during the years 1928–32, and about the same as during 1927. They tended to be high in periods in which incomes and the general price level were low and low when the latter were high. The Commission was obviously greatly disturbed by these vagaries in the behavior of cost of reproduction indices. It had felt obliged, in response to insistent public demand, to order a reduction in rates, and hence was virtually compelled to arrive at a valuation which would justify the reduction. The consequence was the scrapping of the reproduction cost concept and the adoption of the general purchasing power principle instead. These facts are clearly revealed in the following passage from the Commission's decision:

> These numbers (indices of cost of reproduction of telephone property) *indicate a situation so at variance with the experience of people generally* not only of this country but of the world—a situation described by the Supreme Court as "a new experience to the present generation"—*that this Commission feels that they cannot possibly show the actual present-day value of the company's property.* For a relatively low value of the property would be obtained by using the index numbers shown in a time of high prices, 1929 for instance, while relatively high values would be found by using the index numbers given for years when prices were low. This also is contrary to the company's experience, for in 1924 it claimed a higher than cost value for its rate base because prices then were high. It seeks now a high rate base, and an increased rate of return, when prices are low.
>
> Such claims do not seem reasonable to this Commission. . . . *It is an unavoidable consequence of such a condition* (the general fall in prices since 1929) *that the fair value of a property employed in the public service and charged with a public interest should be subject to the general changes in the value of the dollar,* and that its fair value *must* be less than the price which its books show was paid for it in times of higher prices; just as the value of the property was found by the court to be greater in 1924, when prices were high, than the books showed had been paid for it.
>
> . . . . . .

[27] See quotation from Northern Pacific Case, p. 248 *supra*.

> It is apparent to the Commission that if the company were permitted now to earn the same rate of return allowed in 1924 and subsequent years upon a rate base equal to the value of property purchased since in years of high prices, *then its owners would actually profit by the world-wide depression which has, so generally, brought financial loss and distress to the rate-payers.* For while its stockholders would receive their dividends in the same number of dollars, they would get dollars of superior purchasing power. Their real return in bad times, therefore, would be greater than the return in good times, and the longer the depression continued, the greater would their harvest be.[28]

The following passage gives the District Court's answer to the foregoing contentions and is an admirable statement of the ground on which, in the writer's opinion, the Commission was finally overruled by the Supreme Court:[29]

> It is apparent that the Commission in a general way was seeking to value the telephone property on the basis of an *average* trend of cost of materials, labor, and construction, but instead of taking an absolute average trend of all costs it took a so-called weighted average of only certain costs. The difference is, however, in our opinion, not of fundamental importance. In either case there is the essential fallacy of *assuming* that an *average trend of all prices*, whether absolute or weighted, is a true index for the trend of construction costs of the special kind of property with which we are here dealing. Whether the valuation of telephone property is based on historical cost or reproduction new, it is, we think, obvious that the price changes over a period of years can only fairly be reflected by developing a price ratio or trend for the costs entering into this particular kind of property. It is quite erroneous to assume that changes in the price level of certain commodities or the average of any number of commodities is similar to that of a particular commodity or property. . . . If this valuation method be used at all, we think it entirely clear that the only proper use is that adopted by the Company and not that used by the Commission; provided, of course, the cost data used by the Company represented fair and reasonable prices for telephone apparatus and construction work.[30]

For reasons sufficiently elaborated in the foregoing the writer believes that the opinion of the majority of the Supreme Court was correct in the light of the facts in this case. But, as stated at the outset, it is his opinion that the views of the minority are clearly preferable, both legally and economically, if in subsequent litigation it appears that this decision is to stand as a general condemnation of valuations involving the use of index numbers in place of appraisals. On the legal side, it seems to the writer that the wisdom or expediency of using index numbers, and the choice of, and weight to be given to, such numbers are questions of fact which under accepted legal principles

[28] *Re Chesapeake and Potomac Telephone Co. of Baltimore City*, 1 P.U.R. (New Series) 346, at pp. 352–53 (1934). Italics mine.

[29] The distinction between the cost of reproduction and general purchasing power concepts in connection with price indices has been recognized by several commissions: See *Re Coast Gas Co. (New Jersey)*, P.U.R. 1923A, 349, at pp. 353–57; *Re Bluefield Telephone Co. (West Virginia)*, P.U.R. 1926D, 209, at pp. 214–20; *Washington Heights Taxpayers Assn. vs. New York Edison Co. et al. (New York)*, P.U.R. 1932 E, 218, at p. 23; dissenting opinion in *Re The Ohio Bell Telephone Co. (Ohio)*, 2 P.U.R. (New Series) 113, at p. 165 (1934).

[30] *Chesapeake and Potomac Telephone Co. of Baltimore vs. Harold E. West et al., Constituting the Public Service Commission of Maryland*, 3 P.U.R. (New Series) 241, at p. 271 (1934). Italics are the Court's.

are not reviewable by courts.[31] On the economic side, it is likely that a considerable number of economists would agree with Justice Stone both as to the comparative accuracy of index numbers and appraisals in finding reproduction cost and as to the necessity of using the former device if there is to be economical and effective regulation in the present situation, in which reproduction cost is an important or even dominant element in the "fair value" which the commissions are required to find.[32] Concerning the limitations of appraisals, and affording striking support to Justice Stone's position, is the following by Messrs. Bauer and Gold:

> The range of honest and competent differences may not be so great in some cases, but the processes of valuation are always involved with uncertainty as to fact. Appraisal consists extensively and inevitably of assumptions and guesses in planning and installation of properties which already exist and cannot be actually reproduced. *It deals*[33] *largely with theoretical quantities, hypothetical processes and assumed conditions, all determined by judgment of individual experts, instead of concrete facts capable of complete demonstration.* At best it is time-consuming, cumbersome, and expensive. The final determination, though sincerely made by commission, court or other fact-finding body, constitutes only a rough approximation, and not a fact that is definite and has been permanently established for administrative purposes. It is subject to litigation in the particular case, and soon thereafter when conditions have changed, may have to be determined anew.
>
> . . . . . .
>
> The system leads inevitably to elaboration of detail, exaggeration of unimportant items, constant and useless wrangling, artificial and meticulous distinctions, excursions into immaterialities, stressing technicalities, and studied postponements, all at great expense and long delay to final but unconclusive decision.[34]

As examples of the wide variations in the results of cost of reproduction appraisals may be mentioned the case of the Ohio Bell Telephone Company, where the figures ranged from $104,282,735 to $157,000,000; the Union Electric Light and Power Company of St. Louis, where the range was from $40,000,000 to $73,000,000; and the Worcester Electric Lighting Company, where the range was from $9,000,000 to $16,000,000.[35] Likewise, as illustrative of the delay and expense incurred in arriving at valuations involving such appraisals may be mentioned the New York Telephone Company Case, which extended over eleven years (1920–30) and cost the company alone over

[31] But see *Ohio Valley Water Co.* vs. *Ben Avon Borough*, 253 U. S. 287 (1920); *Crowell* vs. *Benson*, 285 U. S. 22 (1932).

[32] See W. A. Prendergast, *Public Utilities and the People* (Appleton-Century, 1933); C. O. Ruggles, "Some Aspects of Public Utility Rate Making," *Harvard Business Review*, 13, 417–34, (1935); W. J. Graham, *Public Utility Valuation—Reproduction Cost as Base for Depreciation and Rate Base Determination* (Monograph, 1934, University of Chicago Press); P. J. Raver, "Index Numbers of Public Utility Construction Costs," *Journal of Land and Public Utility Economics*, 3, 343–60 (November, 1927); F. G. Dorety, "The Function of Reproduction Cost in Public Utility Valuation and Rate Making," *Harvard Law Review*, 37, 173–200 (December, 1923); (with qualifications) Bauer and Gold, *Public Utility Valuation* (Macmillan, 1935).

[33] The word used in the original is "details."

[34] *Op. cit.*, pp. 376, 378. Italics mine.

[35] Mosher and Crawford, *Public Utility Regulation* (Harper and Brothers, 1933), p. 204.

$5,000,000, and the Ohio Bell Telephone Company Case, which lasted ten years (1922–31).[36] Consequently one very important reason why rate control, which necessitates the finding of "fair value," has been rendered largely ineffective, and, in some cases may even be said to have gone by default, is the delay and expense involved in finding reproduction cost, which plays such an important part in the current "fair value" requirements of the Supreme Court.

On the other hand, several writers are skeptical as to whether the use of index numbers in place of appraisals would improve the situation as outlined above.[37] Their criticisms concern the accuracy and appropriateness of price data, the difficulty of taking into account changes in labor efficiency and in technology, and the like. It may be remarked in passing that these criticisms would apply to a majority of appraisals also. Certainly, however, few, if any, available indices achieve the standard which would be necessary for satisfactory application in the important task in question. The writer appreciates fully the difficulties and limitations of index numbers at their best. Nevertheless it is his opinion that reasonably satisfactory index numbers for use in place of appraisals can be constructed, and that the use of such indices is essential to the economical and effective administration of the current requirements for determining "fair value," in which reproduction cost is an important or even dominant element. This is not, of course, in any way to defend either reproduction cost or the current "fair value" requirements.

CONCLUSIONS

To sum up, in the writer's opinion, the majority of the Supreme Court in the case under review were on sound ground both legally and economically in reversing the Maryland Commission. On the legal side, it is the writer's view that the Commission was reversed because, while purporting to find the reproduction cost of the telephone company's property, it had in fact found the company's "real" investment in terms of 1932 dollars. Hence the resulting valuation was dominated by the factor of purchasing power of money in general, a factor which independently is not one of the "values or elements of value" which according to previous decisions of the Supreme Court must be "considered" or "given weight" in arriving at a proper valuation. On the economic side the majority showed insight in distinguishing between indices of the general price level and indices of the cost of reproduction

[36] *Ibid.*, pp. 205–6.

[37] *Ibid.*, pp. 212–13; George E. Goldthwaite, "Reproduction Cost and Falling Price Levels," *National Municipal Review*, 21, 427, at pp. 428–30 (July, 1932); E. C. Goddard, "The Evolution of Cost of Reproduction as the Rate Base," *Harvard Law Review*, 41, 564, at pp. 583–84 (March, 1928).

of telephone property, and in recognizing the diversity of movement shown by these indices.

On the other hand, in the writer's opinion, the minority were clearly correct in their contentions as to the relative accuracy of reproduction cost valuations arrived at by appraisals and by the use of index numbers, respectively, and as to the necessity of using the index number method if there is to be economical and effective regulation in the present situation, in which reproduction cost is an important or even dominant element in the "fair value" which commissions are required to find. The writer believes that the majority were correct in the light of the facts in this case, but that it would be unfortunate if in subsequent litigation it should appear that the Court intends this decision to stand as a general condemnation of valuations arrived at by the use of index numbers in place of appraisals. If the view of the writer, as to the reason for the decision in this case, is correct, this last is not the Court's intention.

DISTINCTIVE CONCEPTS OF PRICE AND PURCHASING-POWER INDEX NUMBERS

By Wirth F. Ferger, *Associate Economic Adviser to the Secretary of Agriculture*

The usual method of measuring changes in the value of money or its purchasing power is to use the reciprocal of the index of the general price level, regarding the value of money as simply the inverse of prices. This usage has apparently followed from the obvious relation between money and goods in the case of a single commodity. If the price of an article doubles, a given amount of money will buy only one-half as much of it: the change in the purchasing-power of money over a given commodity is merely the inverse or the reciprocal of the change in its price. It has been accepted without question that the same principle applies to a composite unit of many commodities, and that an index of the prices of such a group satisfactorily measures (inversely) the value of money as applied to these commodities. It is the purpose of this paper to examine the validity of this transfer of principle. Instead of there being just one problem having two reciprocal aspects, we envision two related but different problems. A price index attacks the relationship of a given composite unit of goods to a varying amount of money: a purchasing-power index attacks the relation of a given amount of money to a varying amount of the composite unit of commodities. The distinction will appear in the weighting systems which are used and the implicit assumptions made when these weights are applied in mathematical formulas.

The discussion concerning index number technique has turned mainly on the selection of data and the algebraic characteristics of various formulas, with insufficient attention being given to the fundamental nature of the phenomena dealt with. Such discussion of methodology is futile unless agreement can be reached as to the purposes to be served. Conversely, a clear conception of the phenomena being studied will usually go far toward solving the problem of technique. These statistical problems should be attacked from the standpoint of the economic principles involved as well as, and precedent to, an examination of the mathematical qualities of index number formulas. We propose to examine in detail the economic nature of the phenomena of prices and money and to measure these characteristics up against the algebraic properties of index number formulas.

I

Before attacking the problem of purchasing-power index numbers, we must first clarify certain basic concepts of price indexes. Our starting-point may well be Bowley's classic definition:

> Index-numbers are used to measure the change in some quantity which we cannot observe directly, which we know to have a definite influence on many other quantities which we can so observe, tending to increase all or diminish all, while this influence is concealed by the action of many causes affecting the separate quantities in various ways.[1]

What Bowley apparently has in mind is an index of the general purchasing power of money—the field in which index numbers were first employed. Several ideas are implicit in this definition: first, that there is a single real and definite phenomenon or value which it is sought to measure.[2] It may be questioned, however, whether any such definite force always exists, whose nature and magnitude it is sought to determine by observing its multiple influences on measurable phenomena. Does an index of the physical volume of production in agriculture or industry have any such connotation of a single central force? It might be answered that "prosperity" is this ultimate object of measurement; but the reply is that this is not a single *force* exerting a uniform influence on many measurable quantities, but is an abstract *resultant* of many diverse forces. It is true that there must always be some unifying idea governing the selection of data to answer the requirement of homogeneity, but Bowley had something much more definite in mind.

In this connection it should be remembered that statisticians recognize two kinds of average, abstract and concrete, or in Jevons' words, fictitious and precise. Abstract or fictitious averages are pure fictions of the imagination—convenient abstractions invented to serve certain functions of summarization and comparison. The average wage of workers in a factory, the average weight of the players on a football team, illustrate this type of average. There is no reality corresponding to the figure for wage or weight obtained by the mathematical process of averaging. If, on the other hand, repeated measurements are made by one or more investigators of the magnitude of an angle, the distance to the moon or the mass of an object in the laboratory, the average of the successive observations is our best estimate of the true magnitude—it represents an actual, real, concrete, existing fact. Now, the theory underlying the choice of method in computing an average depends,

[1] Arthur L. Bowley, *Elements of Statistics*, 3rd ed., p. 217.

[2] Compare C. M. Walsh, *The Measurement of General Exchange Value*, Chap. III, especially pp. 54–67.

among other things, on the nature of the phenomena being averaged, as regards these considerations. In the latter case the theory of errors applies strictly; in the former it applies only by empirical demonstration of analogy.

Bowley's definition obviously regards index numbers as being concrete or real averages, and a long line of students from the time of Jevons have accepted this position and have based their theory of method and technique on this premise.[3] By explicit statement and by direct reference to physical analogy they have defined the problem of an index of the value of money as being of the same nature as the problem of averaging repeated measurements of a physical quantity. They regard price indexes as measuring the definite and uniform influence of monetary changes on all prices, segregated or extracted from the divergent "demand and supply" influences affecting individual commodities.

From this view of the economic nature of the problem of price indexes flow important consequences. Only by such a concept can a search be justified for a "universal" or an "ideal" index number formula, judging perfection by purely mathematical tests.[4] From this view, again, Edgeworth formulated two principles of construction; first, that an index number of the value of money should be unweighted, for each price quotation is an independent "observation" of the basic phenomenon and thus has as much importance as any other, regardless of the volume of trade or consumption of the article.[5] Secondly, he concluded, the proper average to be employed is the median, in order to reduce the influence of those items deviating widely from the central values, thus indicating less reliability as an observation of the real phenomenon, monetary causes of price changes.

A second implication of Bowley's definition is that index numbers involve a sampling process among the "other quantities which we can [directly] observe." Thus it is only necessary to design the methods of construction so as to conform to the mathematical theory of sampling to assure an accurate measurement of the movement of prices and the value of money.

[3] Jevons, Edgeworth, Hawtrey, Flux, March and Gini. See W. Stanley Jevons, "The Variation of Prices and the Value of the Currency since 1782," in *Investigations in Currency and Finance*, 2nd ed., p. 114; F. Y. Edgeworth, "Some New Methods of Measuring Variation in General Prices," *Journal of the Royal Statistical Society*, vol. 51 (1888), p. 348, and numerous other references; R. G. Hawtrey, "Money and Index Numbers," *idem*, vol. 93 (1930), p. 65; A. W. Flux, "The Measurement of Price Changes," *idem*, vol. 84 (1921), pp. 178–179; Lucien March, *Mouvement des Prix et des Salaries pendant la Guerre*, pp. 11, 13; Corrado Gini, "Quelques Considérations au Sujet de la Construction des Nombres Indices des Prix et des Questions Analogues," *Metron*, vol. 4 (1924), p. 15. See also Bowley, "The Measurement of Changes in the Cost of Living," *Journal of the Royal Statistical Society*, vol. 82 (1919), p 345.

[4] Reference is made to Walsh's "universal" formula, op. cit., pp. 375, 376, and to Fisher's "ideal" formula, *The Making of Index Numbers*, e.g., p. 220.

[5] *Memorandum of the British Association for the Advancement of Science*, 1889, p. 157.

II

This point of view is logical, if the premises be granted. It is submitted, however, that an entirely different view must be taken of the problem of price and purchasing-power indexes. It is our contention, first, that neither are individual prices and price changes independent phenomena, nor is their selection for inclusion in an index number made at random. It would seem that these statements need only to be brought to the attention of economists to receive acceptance. The first, that all prices are interdependent, has long been an axiom of economic theory, because of the fundamental principles of scarcity and substitution, and because economic value (of which "price" is a special case) means *exchange* value, a purely relative concept. As to the random character of various prices, we are certainly not justified in assuming that the divergent demand and supply changes of individual commodities occur without reference to their popularity. We cannot assume that the dispersion of the relatives around the median is random and independent of the positions the commodities occupy in the economic system, for this dispersion is caused, even according to the viewpoint taken by the authors referred to, by the individual demand and supply conditions of the commodities. But changes in the demand and supply conditions of one commodity are quite likely to be correlated with such changes in related commodities.[6] As to their sampling selection, it is obvious that the primary consideration for the selection of commodities, both in the past and at present, is the availability of the data on their prices. This selection naturally leans strongly in the direction of the commoner commodities, and violates the criterion of randomness.

A third objection, of less theoretical importance, is the question as to whether, in actual practice, we commonly have a large enough sample to depend on the theory of errors. By the theory of the purchasing power of money over commodities weighted according to their actual importance, a relatively small sample of judiciously selected goods may be quite representative; but it requires a much larger sample, by the theory of errors, to reach the condition where "The rope is much stronger than its component strands."[7]

We would raise a still more fundamental question, however, regarding this view of price indexes. These students explicitly seek in such an index a measure of the *uniform* influence on prices exerted by

[6] Compare Bowley, "The Influence on the Precision of Index-Numbers of Correlation between the Prices of Commodities," *Journal of the Royal Statistical Society*, vol. 89 (1926), pp. 300–319; and Edgeworth, *idem*, vol. 88 (1925), pp. 564, 570, note. While recognising some relationship between prices, it appears that these scholars underestimated its degree or importance in the theory.

[7] Edgeworth, "Mr. Walsh on the Measurement of General Exchange Value," *Economic Journal*, vol. 11 (1901), p. 409.

changes in money, unaffected by the diverse price changes caused by the sundry demand or supply changes of individual commodities. Without any attempt to survey the whole of the broad field of monetary theory and the problems of stabilization implied, we question whether there is, *even in theory*, any such uniform monetary influence.[8] Consider, first, how a "monetary" cause actually works. Theorists have often carelessly assumed that if everyone suddenly awoke some morning with double the money previously possessed, the prices of all commodities would immediately and uniformly double. We see no justification for such an assumption: for one thing, it ignores all contractual relations. Such a view was not held by the originator of the illustration, John Stuart Mill. Mill claimed only that after a sufficient lapse of time to allow for adjustments to be made, "general prices" would respond *as a whole* exactly in proportion to changes in the supply of money, other things being equal.[9] He explicitly stated that the immediate effects on various prices would be quite different. Prices are set solely and exclusively by individual demand and supply conditions, and change variously, even in response to a single stimulus such as inflation, as the people of a society amend their purchasing habits. Differing elasticities of demand and differing stabilities[10] of demand (as well as of supply) connote that as the collective purchasing power of a community changes, prices of various commodities react in quite diverse manners. Even taking the clearest case of a "monetary cause," paper money inflation, the paper money originates in the hands of a particular purchaser, the government, and the inflationary tendency works first through the demand for the commodities and services required by the government. The monetary cause itself *upsets the equilibrium* of the demand for the various types of goods, instead of exerting a uniform influence on all prices.

This leads us to quite a different concept of price and purchasing-power index numbers. Rather than initial changes in the value of money influencing all prices uniformly (and then these individual prices being further modified and masked by changes in their diverse demand and supply factors), actually the changes in the demand and supply relations of individual commodities, and thus their prices, are the only real phenomena that exist, even in theory. There *is* no change in the value of money except that which is the result of, or rather is

[8] Compare J. M. Keynes, *A Treatise on Money*, vol. I, Chap. 6, entitled, "Is there such a thing as an 'Objective Mean Variation of General Prices'?"; Gottfried Haberler, *Der Sinn der Indexzahlen*, pp. 73–76, "Der Geldwertbegriff. Monismus und Pluralismus in der Indextheorie."

[9] *Principles of Political Economy*, Ashley ed., pp. 492, 493.

[10] By stability of demand is meant the tendency for the demand schedule for a commodity to remain relatively constant and unchanging over time. Thus the demand for eggs may be stable, though elastic, while the demand for brown silk stockings may be unstable, although conceivably inelastic.

composed of, changes in the prices of commodities. In theory as well as in practice the value of money is merely the abstract summary statement of its purchasing power over specified commodities and services.[11] Changes in the value of money are merely summarized, by averaging in some appropriate manner the observed changes in its purchasing power over these individual commodities and services—their prices. Since we can no longer regard different prices as independent or random observations of a single real phenomenon, we cannot justify the use of an unweighted average. On the contrary the summary expression of the purchasing power of money must take equal account of all transactions: in other words, each price must be weighted by its proper volume of trade.

Since these indexes are abstractions, it is obvious that there can be no objective test for judging what mathematical formula gives the correct and accurate result in measuring, for example, the value of money. Our inquiry concerning a method must not be, "Is it correct?" but "Is the abstract concept thus defined and measured a useful concept, in accordance with the economic characteristics of the phenomenon under discussion, and thus applicable to the purpose in hand?"[12] Just what are the distinctive characteristics of purchasing-power indexes?

III

Prices, being money ratios, have several aspects, corresponding to the different functions performed by money. We are not here interested in the physical use of money as a medium of exchange: it is the economic aspects of money which claim our attention. On the one hand money serves as a standard of value or a measure of value; on the other hand we may note its function as a standard of deferred payments and a storehouse of value. Prices as measures of value serve merely as a convenient method of comparing values—a suit of clothes is worth $22.50, shoes $8 a pair and bread $.07 a loaf, etc., instead of a suit being worth 2.82 pairs of shoes or 321 loaves of bread, a pair of shoes worth .355 suits or 114 loaves, etc., *ad infinitum*, the value of each being quoted in terms of all the others. No element of time is involved in this use of prices for comparing values. When we come to the other uses of money, however, stating a debt (or deferred payment) in terms of money, or laying aside money against a rainy day,[13] there

[11] Compare Gottfried Haberler, "A New Index Number and its Meaning," *Quarterly Journal of Economics*, vol. 42 (1927–28), p. 435.

[12] Compare the point of view of Willford I. King in his *Index Numbers Elucidated*.

[13] Money thus hoarded can be regarded as a special case of a debt or deferred payment—a debt by society to the saver: a note signed by society rather than by an individual. These two functions of money can therefore be subsumed under the first, a standard of deferred payments. Contrast the treatment of the "standard of deferred payments" by Haberler, *op. cit.*, p. 441.

are other more complicated aspects of money which must be recognized—the buying power of money. In the former instance money serves merely to facilitate the statement of a set of *instantaneous* value relationships; in the latter cases money serves to define and record composite values *over periods of time.*

The writers on monetary problems have always assumed, either implicitly or explicitly, that the problem of the purchasing power of money is identical with the problem of the price level: that an index of the value of money is universally equivalent to the reciprocal of a price index. Thus Edgeworth introduced an article on index numbers with the statement, "Index numbers are used to indicate changes in the value of money."[14] Bowley's definition of an index number previously quoted implies the same: it is a change in the value of money which constitutes the "quantity which we cannot observe directly, which we know to have a definite influence on many other quantities [prices] which we can so observe." Irving Fisher gives perhaps the most explicit statement:

> The purchasing power of money is indicated by the quantities of other goods which a given quantity of money will buy. . . . In short, the purchasing power of money is the reciprocal of the level of prices; so that the study of the purchasing power of money is identical with the study of price levels.[15]

Even though there is this obvious and intimate relation between prices and the value of money, it is our contention that nevertheless the two are not identical, either in meaning or in the technique of measurement. There have been in the past, in fact, a few scattered and hesitant suggestions of a possible differentiation between these two problems. A. W. Flux wrote in 1907,

> There are two important questions to which one might expect to find the answer in the indications of a well-devised and carefully kept index-number. The one is, "What is the change in the money-cost of the things we buy, due to price-changes since any given date in the past?" The second is, "What is the average change in the value of money relative to other things since any given date in the past?" In one sense these are but two forms of one question. But in another sense they are different. The first presents a concrete problem: given a certain mass of commodities, what should they cost, or in what proportions would their cost vary, at different times? It will be readily seen that this differs substantially from the question of how price variations affect the cost of living. The former question ignores, the latter should distinctly include, considerations relating to the changes in standards of living which are developing.[16]

In this quotation Flux goes right up to the threshold of the distinc-

[14] Palgrave's *Dictionary of Political Economy*, vol. II, p. 384.

[15] *The Purchasing Power of Money*, pp. 13, 14. Compare also Walsh, *op. cit.*, pp. 253, 375, 376, and his *The Problem of Estimation*, p. 116; Carl Snyder, *ibid.*, p. 189; Sir Geo. H. Knibbs, *Journal of the American Statistical Association*, Vol. 19 (1924), pp. 200, 201, and *Price Indexes, their Nature and Limitations, the Technique of Computing them, and their Application in Ascertaining the Purchasing-Power of Money*, p. 1.

[16] "Modes of Constructing Index Numbers," *Quarterly Journal of Economics*, vol. 21 (1906–07), pp. 620, 621.

tion, and then does not take the final step. Instead, he implies first that a single "well-devised index number" should answer both questions, although later he seems to realize there must be two formulas, one to ignore, and the other to include changes in the standard of living. However, the most serious deficiency of Flux's analysis is that the distinction he thus makes between the indexes does not coincide with nor answer the two questions he has set up. The difference between price and purchasing-power indexes is not that between fixed and changing standards of living.

The distinction we make between price and purchasing-power indexes rests on another basis. Allyn Young glimpsed the solution in 1921 in his "Measurement of Changes of the General Price Level":

> With constant weighting $\Sigma(t)$ [physical quantities] is constant, and the index numbers express merely the fluctuations of $\Sigma(m)$ [total value]. In this form index numbers of the general price level are the "weighted sums" which several recent writers have ranked among the better index numbers, and which have proved themselves serviceable in practice. But though free from some of the defects of other familiar types of index numbers, these weighted sums are not wholly satisfactory expressions of the general price level. They indicate merely the varying amounts of money payments required by sales of constant quantities of goods at prices determined in part by the condition that such sales have not in fact been constant. Or with other weights than t [quantities], they may indicate the changing market value of a constant volume of production, or the changing cost of a fixed bill of goods or of a fixed standard of living.
>
> Another sort of weighted sum is possible. If $\Sigma(m)$ [total value] and its constituent parts are treated as constants $\Sigma(t)$ [total quantities] would serve as a weighted sum. Its variations would show the fluctuations in the volume of goods that would be sold under the condition that constant amounts of money are assigned to the purchase of each commodity at prices determined in part by the condition that money payments are not, in fact, constant.
>
> The two different types of weighted sums give different, and inconsistent, index numbers. There is no reason to prefer one to the other as an index of the "general level of prices." For this purpose neither is satisfactory. Each has its own significance, but this significance lies in the particular meaning determined by the method of construction. Paucity of data makes the second type impracticable except for the study of limited fields of production, consumption, and trade. But it has a better right than the other to be considered an index of the "purchasing power of money."[17]

In the last sentence of this quotation Young found the secret, we believe, of a purchasing-power index. He did not, however, follow up the analysis or support his viewpoint. In a later writing, in fact, Young reversed his position:

> It has sometimes been urged that the harmonic average is the proper measure of a change of the *purchasing power* of money, since purchasing power is the reciprocal of price, and a change of price is accompanied by an inverse change of purchasing power. The point is not sound. A change in price *is* an inverse change in purchasing power. In a properly constructed index number it should be a matter of indifference whether price changes or their reciprocals are the component units. But the harmonic average does not agree with the arithmetic average. It uniformly gives a smaller result. A glance at its formula will show that the harmonic average of relatives is

[17] *Quarterly Journal of Economics*, vol. 35 (1920–21), pp. 569, 570.

> the reciprocal of the arithmetic average with the base shifted to the other of the two years involved in the comparison. It has the same type of biased error as the arithmetic average, but its error is in the opposite direction. In general, therefore, what has been said of the arithmetic average holds, *mutatis mutandis*, for the harmonic average. For example, just as the arithmetic average exaggerates a general rise of prices so the harmonic average understates it.[18]

We believe Young confused several ideas in this passage. He ignored his former reasoning with regard to the differing weighting systems appropriate to the two indexes, in favor of a purely algebraic approach and a judgment based on ideas of "biased" results. Denouncing both the arithmetic and harmonic averages as having "biased error" begs the question as we are considering it on its economic merits.

IV

As stated by Fisher in the definition quoted above, the purchasing power of money implies "the quantities of other goods which a given quantity of money will buy." *Changes* in this purchasing power, then, would be indicated by the changing quantities of other goods which the same *given quantity* of money will buy. It is to be demonstrated that this does not agree, as is generally assumed, with the reciprocal of the price index. The price index measures how much money is needed at successive times to purchase *the same quantity of a bill of goods.*[19] Its reciprocal is not identical with the true purchasing power of money because a different assumption is implicit as to the weighting system used.

One of the series which it is agreed must be prominent in an index of the general price level is the index of wholesale prices. This will be employed to illustrate the contention here made, namely that what is needed to measure changes in the purchasing power of money is *not* the reciprocal of an index of prices, but *an index of the reciprocals of prices*, properly weighted.[20]

The *price* index weights the various commodities in accordance with the relative quantities used at a given time (usually the base year or

[18] Chapter on "Index Numbers," in *Handbook of Mathematical Statistics*, H. L. Rietz, editor-in-chief p. 185.

[19] This statement would seem at first sight to require a slight modification in the case of double-weighted formulas, such as the so-called "ideal" formula. In this case two different "bills of goods" appear to be used. But Persons has shown in Chapter II of his *The Construction of Index Numbers* that the "ideal" formula in effect employs a constant average bill of goods. Our criticism thus applies strictly, without modification.

[20] Many perplexing problems enter into the determination of the index of this general price level; what commodities and services should be included, what prices (wholesale, retail or both) should be used and in what proportions, and whether wage rates, interest rates and security prices should be included. It is not our purpose to discuss all of these questions, but to concentrate attention on one problem— the weights which should be employed for the various elemental series, and the manner in which these weights should be applied in the formula. For the other problems refer, e.g., to Carl Snyder, "A New Index of the General Price Level from 1875," *Journal of the American Statistical Association*, Vol. 19 (1924), pp. 189–195. See also C. M. Walsh, *The Measurement of General Exchange Value*, Chap IV, pp. 76–135; Haberler, "A New Index Number and its Meaning," *op. cit.*

the average of several years), keeping these *quantities* constant as the prices of the commodities vary.[21] Thus, as time passes, more relative importance (in the budget) is given to those commodities whose prices have increased, and less importance to those commodities whose prices have decreased. Let this be illustrated by the data in Table I, part 1. For convenience, only two commodities are considered, and equal weights are given them—one unit of each being purchased each month. Note, furthermore, that in each month the price changes are assumed to be of opposite direction but of the *same percentage amount.*

TABLE I

COMPUTATION OF PRICE INDEX AND PURCHASING-POWER INDEX

(Data hypothetical)

1. Price Index

Commodity	Price (Dollars per Unit) in January	February	March	April	Percentage Changes J to F	F to M	M to A
A	$1.00	$1.20	$1.08	$1.241	+20	−10	+15
B	1.00	.80	.88	.748	−20	+10	−15
Both	$2.00	$2.00	$1.96	$1.989			
(Reciprocals)	100 (base)	100	102.1	100.5			

2. Purchasing-Power Index

Commodity	Price (Units per Dollar) in January	February	March	April
A	1 unit	.834 units	.926 units	.806 units
B	1 unit	1.250 units	1.137 units	1.336 units
Both	2 units	2.084 units	2.063 units	2.142 units
Index	100 (base)	104.2	103.2	107.1

3. Comparison of Results

	January	February	March	April
Pseudo Purchasing-Power Index:	100	100	102.1	100.5
True Purchasing-Power Index:	100	104.2	103.2	107.1

Let us notice the behavior of the index of the prices of these two commodities. During the first period of change, January to February, the equal but opposite 20 per cent changes just balance, resulting in an unchanged index. This is true because the two commodities were actually of equal importance at the beginning of the change. Thereafter, however, since commodity *A* remains higher priced, equal but opposite percentage changes in price do *not* leave the index unchanged, but it moves in the direction of the prices of commodity *A*, which, being more expensive, really is given more weight and thus dominates the index. Equal original weights of one unit each do not actually result in an equally weighted index number system, but one biased in the direction of the price changes of the commodity or commodities which

[21] That this statement is true even though so-called value-weights are used, is evident when it is remembered that the weighted aggregative and weighted arithmetic-average-of-relatives index formulas give identical results when base year quantities and values are used as weights.

have become relatively more expensive. This index is an index of the *prices* of equal *quantities* of the two commodities, rather than (the inverse of) an index of the purchasing power of money.

Now let us compute a *true* index for the purchasing power of money, assuming exactly the same data on prices, but expressing these prices in amounts obtainable for a dollar. (See Table I, part 2.) In this case we still give the two commodities equal weight, but we *keep* them weighted equally in the sense of spending a dollar for each in each month. Our index must measure how much we get for our money. Let us assume temporarily for convenience that the quantity units are homogeneous—a pound in each case. Noting the results in part 3 of the table, we see that the true change in the purchasing power of money from February to March is $-.9$ per cent instead of $+2.1$ per cent, and from March to April is $+3.8$ per cent instead of -1.5 per cent as indicated by the reciprocal of the price-index.

It is evident that this difference arises from the different weights applied by the formulas despite our proposed intention of maintaining equal weights. The price index gives more weight to the price changes of the expensive article A by assuming constant *quantities* to be bought, while the purchasing-power index gives more weight to the price changes in the cheaper article B, by assuming constant *expenditure* to be maintained. Now the obvious and simple implication of the term "an index of the purchasing power of money" is the literal meaning of its power to purchase—how much of other commodities in general a given unit of money will exchange for. If we are measuring changes in its buying power over several commodities, we must thus measure the changing quantities of these commodities when the *same amounts of money* are paid for them respectively, rather than measuring the varying total cost of the same respective quantities.

The formula for the above unweighted, or equal-weighted index of purchasing power may be written as follows, using the notation of p for price in terms of dollars per unit, u (equals $1/p$) for price stated as units per dollar, zero subscripts for base year, and "one" subscripts for given year prices in either form:

$$\text{Unweighted Purchasing-Power Index} = \frac{\sum u_1}{\sum u_0} = \frac{\sum 1/p_1}{\sum 1/p_0}. \quad (1)$$

Let us now examine the construction of the *weighted* index of the purchasing power of money. It is noted in the example given above that the two commodities were equally weighted not only in the sense of the same quantities being used in the base year, but also in the sense of the same value. This limitation must be removed. Further-

more, in actual practice, adjustment must be allowed for non-homogeneous units, yards, tons, quarts, etc. The problem in Table II will illustrate all of these points.

TABLE II

COMPUTATION OF PRICE INDEX—UNEQUAL WEIGHTS
(Data hypothetical)

Commodity	Quantity used per month in 1925 q_0	Price: dollars per unit		Value in 1925 v_0
		1925 p_0	1926 p_1	
Butter	10 lbs.	$.50	$1.00	$5.00
Milk	50 qts.	.10	.20	5.00
Coal	50 lbs.	.02	.01	1.00
Cloth	10 yds.	.10	.05	1.00

To calculate the purchasing-power index, let us first change the form of the prices to the form of units per dollar. These are shown in the second and third columns (u_0 and u_1) of Table III.

TABLE III

COMPUTATION OF PURCHASING-POWER INDEX—DATA IDENTICAL WITH TABLE II

	Price: units per dollar in		Weights	Weighted quantities per dollar in	
	1925 u_0	1926 u_1	$\frac{v_0}{u_0}$	1925 $\frac{v_0}{u_0} \cdot u_0$	1926 $\frac{v_0}{u_0} \cdot u_1$
Butter	2 lbs.	1 lb.	2.50	5.0	2.5
Milk	10 qts.	5 qts.	.50	5.0	2.5
Coal	50 lbs.	100 lbs.	.02	1.0	2.0
Cloth	10 yds.	20 yds.	.10	1.0	2.0
				12.0	9.0

Now it is evident from the "value in 1925" column in Table II that the first two commodities, butter and milk, must be given the same relative weight, and five times the weight given each of the last two articles, reflecting the relative purchasing power expended on them in the base year, 1925. But these weights of 5, 5, 1, and 1 cannot be used directly, for a modifying or adjusting weight must be introduced to correct for the relative size of the physical unit employed in the u's. Specifically, we cannot weight 2 lbs. of butter and 10 qts. of milk (to the dollar) by 5 each, and get a correct result: suppose we called the 10 qts. of milk 20 pts.; then this arbitrary choice of units would entirely change the index. Similarly tons might as logically (perhaps more so) be used to measure the price of coal: a pound of coal has an entirely different significance from a pound of butter. A perfect corrective factor for the weights is the price (dollars per unit) in the base year, p_0, or $1/u_0$. The complete weights are, then, v_0/u_0, which give an

accurate measure of the importance of the commodities when their prices are stated in u's—so many units to the dollar.

From Table III it is evident that the Index of the PurchasingPower of Money for 1926, relative to 1925 is $9.0/12.0 = 75$ per cent.[22] The formula for this true index of the value of money follows: the quantities in the parentheses in the first fraction represent the weights applied to the prices—prices in the form of units per dollar:

$$\frac{\sum (v_0/u_0)u_1}{\sum (v_0/u_0)u_0} = \frac{\sum (u_1/u_0)v_0}{\sum v_0}. \tag{2}$$

This will readily be recognized as a weighted arithmetic average-of-relatives index, using base year value weights (v_0). As is obvious in the second fraction, the relatives represent, as desired in a purchasing-power index, the amounts purchasable for $1 in the given year, relative to the base year (u_1/u_0).

For convenience in using the purchasing-power formula for prices stated in the usual manner of dollars per unit (p's), we may state the formula in the following form:

$$\frac{\sum (u_1/u_0)v_0}{\sum v_0} = 1 \Big/ \left(\frac{\sum v_0}{\sum (p_0/p_1)v_0}\right). \tag{3}$$

This form of the index is especially interesting, as it introduces a second method of viewing the whole matter of formula and weights: this purchasing-power index is simply the inverse or reciprocal of the weighted *harmonic* mean of prices in their usual form. Let us examine the problem from this point of view.

As the writer has shown in another place,[23] the principal use of the harmonic mean is in certain cases of averaging rates or ratios. Ratios may always be stated in two forms, keeping one or the other of the factors constant (e.g., cents per *pound*, or pounds per *dollar*). The conditions of the problem for which an average is sought will determine which of the factors *should* be kept constant (whether in the two periods weights are to be constant in the sense of equal *quantities* bought or of equal *dollars* spent on each commodity). If it is desired to keep constant that factor which *is* constant in the rates as stated, then the arithmetic mean should be used; if, on the contrary, the

[22] For comparison, it may be noted that the *price* index 1926/1925 equals 175 per cent, and the pseudo-index of purchasing power, its reciprocal, equals 57.1 (compared with the true index of 75). It is obvious that the price changes here assumed are extreme—that this example exaggerates the differences which would ordinarily be found in practice.

[23] "The Nature and Use of the Harmonic Mean," *Journal of the American Statistical Association*, Vol. 26 (1931), pp. 36–40.

recorded rates (prices) make variable the factor desired to be constant, then the harmonic mean is the correct average to be employed. Accepting the ordinary method of stating prices as dollars per unit, if we desire to maintain constant relative expenditure, the use of the harmonic mean of price relatives is thus indicated. Taking base year expenditures (values) as weights, the formula is, for the weighted harmonic "price" index,

$$\frac{\sum v_0}{\sum v_0/(p_1/p_0)} = \frac{\sum v_0}{\sum (p_0/p_1)v_0}. \tag{4}$$

This is seen to be the reciprocal of the true purchasing-power index found above and given in formula (3).

V

Our position is, then, that a purchasing-power-of-money index must be so calculated that the proportional *expenditure* on the various commodities is maintained, rather than assuming constant *quantities* purchased. What is the practical significance of this concept? It has a definite meaning, distinct from the usual implication of a purchasing-power index as merely an inverse method of stating price changes. The difference lies in the several functions of money previously referred to. People using money simply as a standard of value and a medium of exchange are interested in *prices* as such. To manufacturers and merchants engaged in the fabrication and trading of goods and services (aside from the obtaining or granting of credit), prices are of interest and importance merely as statements of comparative values at the time—money as a standard of value with no element of speculation. When we pass to the use of money as the standard of deferred payments, however, we are interested in individual prices only as they inversely measure the profit or loss from the speculative[24] transactions involved—the increase or decrease in the purchasing power of money. When long-time contracts are considered, when debt repayments, savings and fixed incomes are the subject-matter, it is the purchasing power *of a given sum* which is directly of interest—the *variations in the amount of goods* the money will buy. Since this measure differs (as the harmonic from the arithmetic mean) from the inverse of the measure of the varying amount of money needed to buy a given quantity of goods, it is evident a choice must be made between the two means—between the two weighting systems implicit in the two averages.

[24] The word "speculative" is here used in its broadest meaning. All time-spanning ownership of property involves speculation—either speculation in an individual commodity or, in the case of the ownership of money, speculation in *all* commodities.

The two uses of money seem to give the clue for the decision. A price index must measure the varying amounts needed to buy a fixed inventory of goods (necessitating the use of the arithmetic mean of ordinary price relatives); a purchasing-power index must measure the varying amounts of goods (in a given monetary proportion to each other) purchasable with a fixed amount of money. It is only the weighted *harmonic* mean of ordinary price relatives whose reciprocal measures the true purchasing power of money.

THE GEOGRAPHIC DISTRIBUTION OF INTRINSIC NATURAL INCREASE IN THE UNITED STATES, AND AN EXAMINATION OF THE RELATION BETWEEN SEVERAL MEASURES OF NET REPRODUCTIVITY*

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PART I—NUMERICAL DATA

The vital statistics of the United States for the year of the last census enable us to compute separately the "true" or "intrinsic" rates of natural increase for each of the States, except Maine, South Dakota and Texas.[1]

The results of these computations, as based on the white female population, are exhibited in Table 1, and, graphically, by graded shading, in the map Fig. 1. Ten States show high figures of over 10 per

GEOGRAPHIC VARIATION IN THE TRUE RATE OF NATURAL INCREASE
White Females in the United States, 1930

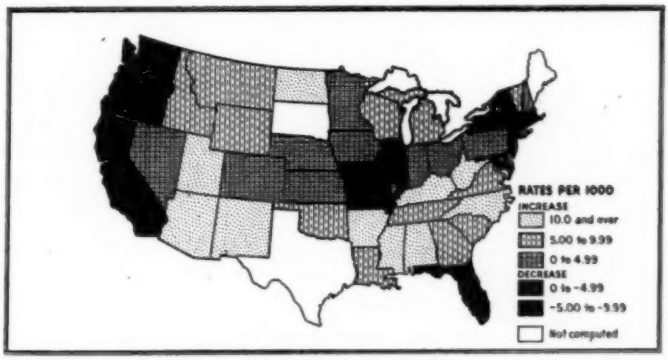

Fig. 1.

1,000 in the true rate of natural increase. Of these ten, six are Southern States. The four States with the highest rates of natural increase are Arizona with 12.69 per 1,000; Utah, 13.26; North Dakota, 13.29; and New Mexico, 18.28. It is to be noted that New Mexico and Arizona have a relatively large Mexican population (here included in the

* Revision of a paper presented to the Ninety-seventh Annual Meeting of the American Statistical Association, New York City, December 31, 1935.

[1] Separate life tables for each of the States, as required for this purpose, have been computed in the Statistical Bureau of the Metropolitan Life Insurance Company, and are published in a volume *Length of Life—A Study of the Life Table*, by Louis I. Dublin and Alfred J. Lotka, Ronald Press, 1936.

"white" statistics) with birth rates presumably above the average of the white population.

Twelve States still show favorable rates, namely, within the range of 5 to 10 per 1,000. Of these twelve, again six are Southern States. The last group of States showing a positive rate of natural increase, namely 0 to 5 per 1,000, includes eleven States. Practically all of these are concentrated in the northern half of the United States.

The remaining States for which computations are available show a deficiency, a negative rate of natural increase. Nine States have a true rate of natural decrease ranging from 0 to 5. All but two of these, Illinois and Missouri, are located on the coast line of the United States. Only one, Washington, is on the Pacific Coast and one, Florida, is in the South. The remaining five are in the industrialized northeastern area of the country. Lastly, the most unfavorable situation, with true rates of natural decrease between 5 and 10 per 1,000, is exhibited in the map by the black areas, including New York, Oregon, and California, which are also coast line States.

If we measure the degree of economic activity by the per cent of the white male population of age 10 and over engaged in manufacturing and mechanical industries, we see in Table 1, that, generally speaking, the States with the high percentages are also the States with true rates of natural decrease or with low true rates of natural increase. The degree of interdependence of the two factors is indicated by the coefficient of correlation, which figures out to $-.65 \pm .09$.[2] Such a figure represents a sizeable correlation, such as, for example, that between body weight and size (length) at birth. A closer correlation, namely, $-.80 \pm .05$ is found when the true rate of natural increase is compared with the degree of urbanization, this latter measured by the proportion of the total population that is contained in municipalities of 2,500 or more inhabitants. It will be noted that the last twenty States on the list in Table 1 all have a degree of industrialization, and also of urbanization, below the average for the United States. Varying social composition (and perhaps other factors) may in part be responsible for the correlation observed.

RELATION BETWEEN THE REPLACEMENT INDEX AND THE TRUE RATE OF NATURAL INCREASE

The data collected in the investigation of the geographic distribution of our intrinsic natural increase furnish convenient material for examining the nature and relationship of three quantities that have been brought forward as measures of the effective fertility in a population.

[2] Throughout this article figures preceded by a plus and minus sign indicate standard errors.

TABLE I

TRUE RATE OF NATURAL INCREASE,* NET REPRODUCTIVITY,* AND REPLACEMENT INDEX, IN RELATION TO DEGREE OF INDUSTRIALIZATION AND URBANIZATION; UNITED STATES, INDIVIDUAL STATES,† 1929–1931

State	True rate of natural increase* per 1,000, ρ	Net reproductivity,* ratio of total births in two successive generations, R_0	Replacement index based on children under one year old to women ages 20 to 45 (white) J	Per cent of population ages 10 and over engaged in manufacturing and mechanical industries‡ White Males	Per cent of population contained in municipalities of 2,500 or more inhabitants‡ (Total persons)
(1)	(2)	(3)	(4)	(5)	(6)
Oregon	−6.90	.827	.834	23.5	51.3
California	−6.08	.849	.827	23.9	73.3
New York	−6.06	.842	.869	30.1	83.6
Washington	−4.44	.885	.897	25.6	56.6
Illinois	−4.43	.883	.911	28.1	73.9
New Jersey	−3.86	.896	.915	34.9	82.6
Massachusetts	−2.99	.917	.933	36.1	90.2
Florida	−2.94	.923	.977	19.8	51.7
Missouri	−2.46	.934	.962	20.3	51.2
Rhode Island	−2.45	.931	.953	41.4	92.4
Connecticut	−1.88	.948	.951	38.8	70.4
Maryland	−1.40	.962	.983	28.3	59.8
Ohio	0.02	1.000	1.015	32.1	67.8
Minnesota	1.10	1.033	1.054	17.4	49.0
New Hampshire	1.44	1.041	1.038	36.6	58.7
Indiana	1.52	1.043	1.070	28.0	55.5
Iowa	2.06	1.060	1.086	15.4	39.6
Delaware	2.10	1.060	1.046	29.4	51.7
Pennsylvania	2.23	1.065	1.108	30.7	67.8
Kansas	2.73	1.079	1.110	15.3	38.8
United States§	2.82	1.079		25.5	56.2
Colorado	3.20	1.091	1.097	16.9	50.2
Nevada	3.45	1.096	1.087	18.1	37.8
Nebraska	3.66	1.110	1.147	13.1	35.3
Wisconsin	5.02	1.153	1.173	26.3	52.9
Michigan	5.22	1.155	1.198	35.2	68.2
Louisiana	6.44	1.195	1.306	17.6	39.7
Vermont	6.73	1.207	1.218	25.0	33.0
Montana	6.79	1.209	1.243	13.7	33.7
Virginia	7.22	1.227	1.302	19.2	32.4
Wyoming	7.46	1.229	1.269	15.1	31.1
Tennessee	8.30	1.260	1.335	16.1	34.3
South Carolina	8.42	1.267	1.374	22.1	21.3
Oklahoma	8.90	1.274	1.371	14.4	34.3
Georgia	9.13	1.291	1.375	16.9	30.8
Idaho	9.45	1.299	1.360	13.6	29.1
North Carolina	11.29	1.380	1.514	20.2	25.5
Kentucky	11.49	1.375	1.430	13.7	30.6
Alabama	11.52	1.379	1.550	16.3	28.1
Mississippi	11.58	1.380	1.476	10.9	16.9
West Virginia	11.68	1.383	1.511	18.8	28.4
Arkansas	12.52	1.411	1.464	10.7	20.6
Arizona	12.69	1.408	1.412	18.4	34.4
Utah	13.26	1.460	1.524	17.6	52.4
North Dakota	13.29	1.478	1.566	7.4	16.6
New Mexico	18.28	1.649	1.711	13.0	25.2

* The true rate of natural increase and the net reproductivity are computed on the basis of the statistics for white females.

† Exclusive of Maine, South Dakota and Texas, for which the requisite data were not available.

‡ According to census of April 1, 1930.

§ Excludes Texas and South Dakota; includes District of Columbia.

I refer to the replacement index, on the one hand, and to the true or intrinsic rate of natural increase, or the corresponding net reproductivity, that is, the ratio of total births in two successive generations, on the other.

Replacement index. The replacement index is formed by computing for the actual, and separately for the life table age distribution, the ratio of children under a given age, such as five years, to women in the childbearing ages, for example, ages 20 to 45.[3] Having computed these two ratios, the quotient is formed with the ratio in the actual population as numerator, and the ratio in the life table population as denominator.

RELATION BETWEEN
REPLACEMENT INDEX* AND TRUE RATE OF NATURAL INCREASE
FOR 45 OF THE UNITED STATES, WHITE FEMALES, 1930

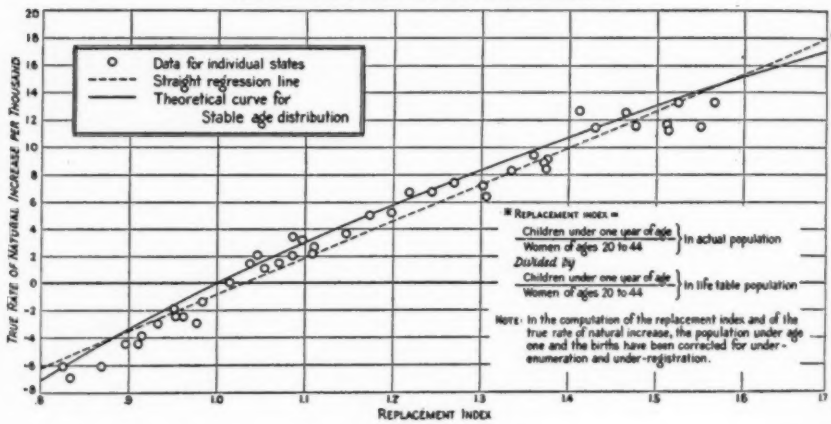
Data for individual states
Straight regression line
Theoretical curve for Stable age distribution
TRUE RATE OF NATURAL INCREASE PER THOUSAND
REPLACEMENT INDEX
* REPLACEMENT INDEX =
Children under one year of age / Women of ages 20 to 44 } In actual population
Divided by
Children under one year of age / Women of ages 20 to 44 } In life table population
NOTE: In the computation of the replacement index and of the true rate of natural increase, the population under age one and the births have been corrected for under-enumeration and under-registration.

FIG. 2.

True rate of natural increase. The true rate of natural increase is that which would result from the prevailing age schedule of mortality and of fertility, if these conditions were allowed to operate for a sufficient length of time to establish their own characteristic stable age distribution. The method of computation of the true rate and the stable age distribution has been described in an earlier publication.[4]

Comparison of replacement index and true rate of natural increase. In the graph Fig. 2, there have been plotted as small circles the values of the true rate of natural increase, for the several States, as ordinates, against the corresponding values of the replacement index as abscissae. Correction has been made in each case for under-enumeration of the population in the first year of life, and for under-registration of births.

[3] Throughout this article age groups are defined in terms of "exact age"; that is, the lower limit corresponds to "age last birthday"; the upper to "age next birthday."

[4] *Journal of the American Statistical Association*, 1925, vol. 20, p. 329.

We can, following the routine procedure, compute a straight regression line and a coefficient of correlation connecting the two variates. We thus find for the coefficient of linear correlation the value of $.985 \pm .005$ and for the regression equation of the true rate ρ of natural increase, against the replacement index J

$$\rho = .02696\, J - .02776. \tag{1}$$

But this is a rather crude procedure. Instead of accepting the relation between the two variates as an empirical fact, and fitting a straight line to the scatter of points by least squares, as shown by line of dashes in Fig. 2 we can seek to establish a rational connection between the variates, and, accordingly, to determine the curve representing this connection.

Now in the perfectly general case, with a purely arbitrary age distribution of the population, there would be no determinate relation at all between the replacement index on the one hand, and the true rate of natural increase on the other. In order to obtain a determinate relation we must assume some definite age distribution. On the assumption of the *stable* age distribution corresponding to the true rate of natural increase ρ, the replacement index has been computed for a number of different values of ρ. The resulting curve is shown in Fig. 2 by a solid line. As will be seen this curve fits the data very well.

This agreement of the computed curve with the scatter of points is at first sight surprising, for the actual age distribution differs quite markedly from the stable distribution, as is clearly evident from the chart Fig. 3. Also, the curve is drawn on the basis of the life table (1929–1931) for the population of the United States as a whole, whereas the points for the individual States have been computed on the basis of separate life tables, one for each State. The differences between these individual tables are in some cases considerable. Thus the expectation of life of white females at birth ranges between 52.22 years for New Mexico and 66.81 years for South Dakota. The reason for the good fit of the computed curve in spite of these differences is examined and explained in Part II.

Relation between replacement index and net reproductivity. In the regular process for computing the true rate of natural increase, there is obtained as an intermediate result the ratio R_0 of births in two successive generations under constant and prescribed conditions of age-specific mortality and fertility. This ratio R_0, which may be termed the "net reproductivity," is defined by the equation,

$$R_0 = \int_0^\infty p(a)m(a)da \tag{2}$$

in which $p(a)$ denotes the probability at birth of reaching age a, in the usual actuarial notation written as l_z/l_0; the symbol $m(a)$ denotes the age-specific fertility of women of age a, counting births of daughters only. The integral or summation may be extended over all ages of life or, which amounts to the same thing, only over the reproductive period, since outside of this period the factor $m(a)$ is zero.

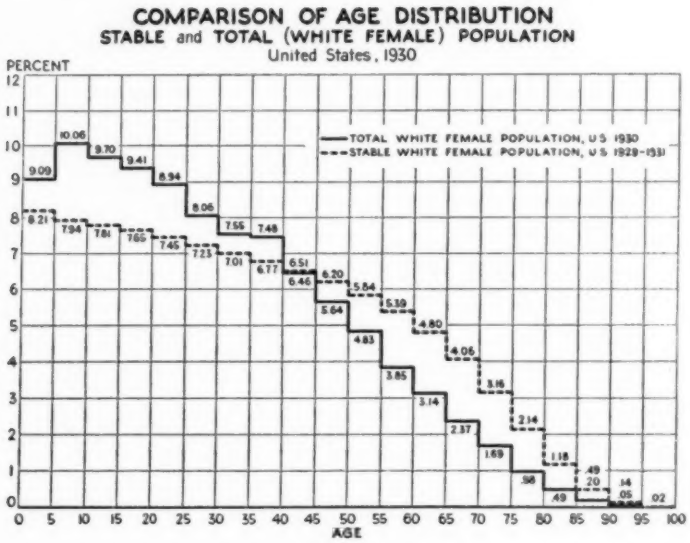
COMPARISON OF AGE DISTRIBUTION
STABLE and TOTAL (WHITE FEMALE) POPULATION
United States, 1930
PERCENT
AGE
TOTAL WHITE FEMALE POPULATION, U S 1930
STABLE WHITE FEMALE POPULATION, U S 1929-1931

FIG. 3.

Figure 4, based on columns (3) and (4) of Table 1, shows the scatter of points representing the replacement index and the corresponding net reproductivity[5] for each of the States of the Union, and a straight line (shown in dashes) fitted empirically by least squares to this scatter, as well as the rationally fitted curve (shown solid) computed on the assumption of the stable age distribution. The regression equation is

$$R_0 = .8654J + .1156. \tag{3}$$

PART II. DEVELOPMENT AND DISCUSSION OF FORMULAE

Relation between replacement index J and true rate ρ of natural increase in a population with stable age distribution. The replacement index J is defined by

$$J = \frac{\int_p^q c(a)da}{\int_u^v c(a)da} \div \frac{b_0\int_p^q p(a)da}{b_0\int_u^v p(a)da} \tag{4}$$

[5] Values of the net reproductivity for the several States of the Union have also been published by B. D. Karpinos in *Social Forces*, 1935, vol. 14, p. 218. His values, however, are not corrected for unregistered births.

where $c(a)$ is the coefficient of age distribution, such that $c(a)da$ gives the proportion of the actual population comprised within the age limits a and $a+da$, and $p(a)$ is the probability at birth of surviving to age a; p and q are respectively the lower and the upper limit of the junior age group which may be taken as comprising both sexes, and u and v are the corresponding lower and upper limits of the senior age

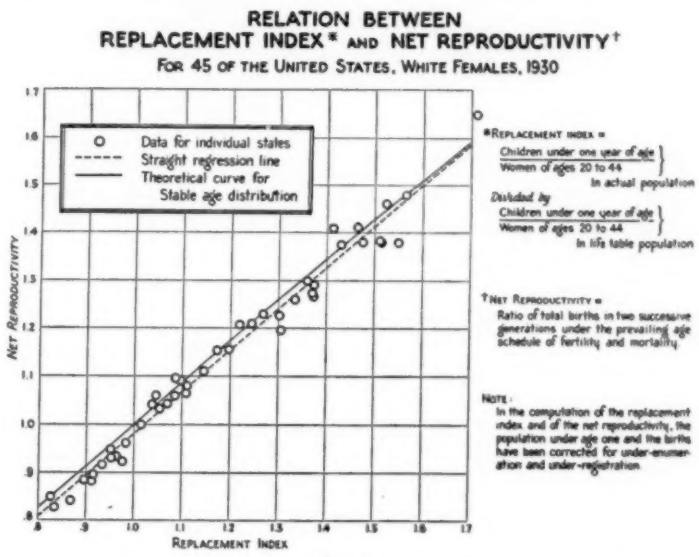
RELATION BETWEEN
REPLACEMENT INDEX* AND NET REPRODUCTIVITY†
FOR 45 OF THE UNITED STATES, WHITE FEMALES, 1930
Data for individual states
Straight regression line
Theoretical curve for Stable age distribution
NET REPRODUCTIVITY
REPLACEMENT INDEX
*REPLACEMENT INDEX =
Children under one year of age / Women of ages 20 to 44 } In actual population
Deluded by
Children under one year of age / Women of ages 20 to 44 } In life table population
†NET REPRODUCTIVITY =
Ratio of total births in two successive generations under the prevailing age schedule of fertility and mortality
NOTE:
In the computation of the replacement index and of the net reproductivity, the population under age one and the births have been corrected for under-enumeration and under-registration

FIG. 4.

group of females, which, in particular, may be the group within the reproductive age period.[6] The symbol b_0 denotes the birth rate per head in the life table population, in which the coefficient of age distribution is given by

$$c(a) = b_0 p(a). \tag{5}$$

In the case of a population with stable age distribution, and with true rate of natural increase ρ and birth rate per head b_ρ, the coefficient $c(a)$takes the form

$$c_\rho(a) = b_\rho e^{-\rho a} p(a) \tag{6}$$

so that the replacement index becomes

$$J = \frac{\int_p^q e^{-\rho a} p(a)da}{\int_u^v e^{-\rho a} p(a)da} \div \frac{\int_p^q p(a)da}{\int_u^v p(a)da}. \tag{7}$$

Let us put

$$y = y_\rho = \int_p^q e^{-\rho a} p(a)da \tag{8}$$

[6] It will be shown in a later section how this condition can be generalised.

so that

$$y_0 = \int_p^q p(a)da \tag{9}$$

and similarly

$$z = z_\rho = \int_u^v e^{-\rho a}p(a)da \tag{10}$$

$$z_0 = \int_u^v p(a)da. \tag{11}$$

Then

$$J = (y/y_0) \div (z/z_0) \tag{12}$$

$$\log J = \log (y/y_0) - \log (z/z_0). \tag{13}$$

Furthermore,

$$\frac{dy}{d\rho} = -\int_p^q ae^{-\rho a}p(a)da \tag{14}$$

$$= -A\int_p^q e^{-\rho a}p(a)da \tag{15}$$

$$= -Ay \tag{16}$$

$$\log_e (y/y_0) = -\int_0^\rho Ad\rho \tag{17}$$

where

$$A = \int_p^q ae^{-\rho a}p(a)da \Big/ \int_p^q e^{-\rho a}p(a)da \tag{18}$$

$$= \alpha_1 + \beta_1\rho + \cdots. \tag{19}$$

The coefficients α_1, β_1, are obtained by expanding in the exponentials $e^{-\rho a}$ under the integral signs, whereby series containing the moments of $p(a)$ are obtained thus

$$A = \frac{M_1 - M_2\rho + \cdots}{M_0 - M_1\rho + \cdots} \tag{20}$$

where

$$M_n = \int_p^q a^n p(a)da \tag{21}$$

and where α_1, β_1 are respectively the mean and the square of the standard deviation taken with negative sign, of the function $p(a)$ taken between the limits p and q, that is,

$$\alpha_1 = M_1/M_0 \tag{22}$$

$$\beta_1 = \alpha_1^2 - M_2/M_0. \tag{23}$$

Integrating (17) after introducing the expression (19) for A, we have

$$\log_e (y/y_0) = -(\alpha_1\rho + \tfrac{1}{2}\beta_1\rho^2 + \cdots) \tag{24}$$

and similarly

$$\log_e (z/z_0) = -(\alpha_2\rho + \tfrac{1}{2}\beta_2\rho^2 + \cdots) \tag{25}$$

the coefficients α_2, $\beta_2 \cdots$ being exactly analogous to α_1, $\beta_1 \cdots$, except that the integrals (moments) are taken between the limits of age u and v (senior age group) instead of p and q (junior age group).

Finally, introducing (24), (25) in (13), we have

$$\log_e J = (\alpha_2 - \alpha_1)\rho + \tfrac{1}{2}(\beta_2 - \beta_1)\rho^2 + \cdots \tag{26}$$

or, if ρ and $(\beta_2 - \beta_1)$ are sufficiently small, and the terms of second and higher degree are negligible,

$$\log_e J = (\alpha_2 - \alpha_1)\rho. \tag{27}$$

The results obtained by these formulae agree very well with those computed directly from the corresponding stable age distribution, as will be seen from the data in tiers II, III and IV of Table 2. This table also exhibits a number of other features which will be referred to later, and which, for convenience in summarizing the results, have been collected together here in one table.

Relation between replacement index and birth rate "per female in reproductive age group" in a population with stable age distribution. If the lower age limit of the junior age group is zero, while its upper limit is brought down closer and closer to the lower age limit, then the mean age α_1 itself, of the group, approaches zero, and J approaches the value

$$\log_e J = \alpha_2\rho. \tag{28}$$

This form of the relation has a certain special interest, for in this case the definition (4) of J becomes

$$J = \frac{c(0)}{\int_u^v c(a)da} \div \frac{b_0 p(0)}{b_0 \int_u^v p(a)da} = \frac{b_\rho}{\int_u^v c(a)da} \div \frac{b_0}{b_0 \int_u^v p(a)da} \tag{29}$$

that is, the replacement index is in this case formed by taking the ratio of the annual births (instead of the population in the junior age group)

TABLE 2

RELATION BETWEEN NET REPRODUCTIVITY R_0 AND REPLACEMENT INDEX J, FOR SELECTED VALUES OF THE TRUE RATE OF NATURAL INCREASE, BASED ON AGE-SPECIFIC REPRODUCTIVE RATES FOR WHITE FEMALES IN THE UNITED STATES, 1920 AND 1930, AND ON LIFE TABLES FOR WHITE FEMALES, 1919–1920 AND 1929–1931

Basic data	True rate ρ of natural increase per head*			
	−.04516	.00265	.02304	.03640
I	Net reproductivity, R_0			
Fertility of 1930 multiplied by appropriate† factor				
Life table of 1929–1931	.269	1.077	1.885	2.692
Life table of 1919–1920	.271	1.077	1.880	2.681
Fertility of 1920 multiplied by appropriate† factor				
Life table of 1919–1920	.264	1.078	1.904	2.735
II	Replacement index J computed from stable age distribution			
Life table of 1929–1931	.248	1.082	1.958	2.856
Life table of 1919–1920	.250	1.081	1.952	2.843
III	Replacement index J‡ computed from true rate of natural increase using first two terms of equation (26)			
Life table of 1929–1931	.247	1.082	1.961	2.863
Life table of 1919–1920	.249	1.082	1.955	2.849
IV	Replacement index J‡ computed from true rate of natural increase using equation (27)			
Life table of 1929–1931	.260	1.082	1.987	2.959
Life table of 1919–1920	.262	1.082	1.981	2.945
V	Replacement index J‡ computed from net reproductivity, by equation (33)			
Fertility of 1930 multiplied by appropriate† factor				
Life table of 1929–1931	.248	1.082	1.961	2.866
Life table of 1919–1920	.248	1.082	1.962	2.866
Fertility of 1920 multiplied by appropriate† factor				
Life table of 1919–1920	.250	1.082	1.956	2.854

* The values of the true rate of natural increase in these four columns correspond respectively to the "multiples" .25; 1.00; 1.75 and 2.50 times the net fertility of 1930 (see first line of table).

† These "factors" by which the age-specific fertilities are multiplied are so chosen as to result in the values of ρ appearing at the head of respective columns.

‡ The values of the constants used in the formulae for the replacement index J are as follows:

Basic data	Age group 0–5		Age group 20–45		Net fertility curve	
	Mean age α_1	Standard deviation squared $-\beta_1$	Mean age α_2	Standard deviation squared $-\beta_2$	Mean age α	Standard deviation squared $-\beta$
Life table of 1929–1931 Fertility of 1930	2.483	2.26	32.285	52.08	28.036	45.38
Life table of 1919–1920 Fertility of 1930	2.473	2.27	32.146	52.09	27.914	45.01
Fertility of 1920					28.470	45.39

to the (female) population in the senior (reproductive) age group, as computed separately for the actual population and for the life table population, and forming the quotient of these two ratios. It is to be

noted that the ratio of the annual births to the female population in the reproductive age group is in itself a figure of interest. It is a "birth rate per head" referred not to the entire population, but to that part of the female population which is actually the source of these births.

Relation between the replacement index J and the net reproductivity R_0 (ratio of total births in two successive generations) in a population with stable age distribution. By a process very similar to that by which equation (24) was developed, it is readily shown[7] that

$$\log_e R_0 = \alpha\rho + \tfrac{1}{2}\beta\rho^2 + \cdots \tag{30}$$

where the coefficients $\alpha, \beta, \cdots$ without subscripts are respectively the mean, and the standard deviation squared taken with a negative sign, of the age-specific net reproductivity $p(a)m(a)$ of the female population.

Then, in view of (26) and (30),

$$\frac{\log_e J}{\log_e R_0} = \frac{(\alpha_2 - \alpha_1) + \tfrac{1}{2}(\beta_2 - \beta_1)\rho + \cdots}{\alpha + \tfrac{1}{2}\beta\rho + \cdots} \tag{31}$$

or, when second degree and higher terms can be neglected,

$$\log_e J/\log_e R_0 = (\alpha_2 - \alpha_1)/\alpha \tag{32}$$

$$J = R_0^{(\alpha_2-\alpha_1)/\alpha}. \tag{33}$$

The results obtained by this formula agree very well with those computed from the corresponding age distribution, as will be seen from the second and fifth tiers in Table 2.

Numerical values and effect of variation of constants. In order to follow intelligently the implications of our formulae, it is desirable to bear in mind the range of numerical values that in practice are likely to be assumed by the various constants.

The net reproductivity R_0 will in modern populations not be very widely different from unity. In historical cases going back a century or more, R_0 may rise to values approaching or even exceeding 2, but in most civilized countries today R_0 is either just over one or a little less than one. In the exponent formed by the fraction $(\alpha_2-\alpha_1)/\alpha$ the mean α_1 of the childhood age group in the life table population will be of the order of two and a half or less, according as the age group under 5 or a narrower one is employed. The mean α_2 of the senior age group, if taken from age 20 to age 45, in the life table population will be usually of the order of 32 years, so that $\alpha_2-\alpha_1$ is of the order of 30.

[7] *Journal of American Statistical Association*, 1925, vol. 20, pp. 329–332.

On the other hand α in the denominator, the mean of the net reproductivity curve of the women, is of the order of 28. The upshot of this is, that the exponent is of the order of unity.[8] In some of the examples presented herewith its value ranges between 1.04 and 1.06.

Inasmuch as this exponent is not very far from unity, we have as a first rough approximation that, with suitable choice of age limits for the junior and the senior age groups, the replacement index J is numerically equal to the net reproductivity R_0 in a population with stable age distribution. It should be noted, however, that this is true only in a somewhat roughly approximate sense, except when R_0 is unity, in which case the replacement index J is also exactly unity for a population with stable age distribution, as it should be, since the stable age distribution in this case reduces simply to the life table age distribution, and the numerator and denominator in the quotient defining J become equal.

It is of particular interest to note what would be the effect, upon the replacement index, of a change, for example, from one life table to another, or from one fertility curve to another. From the nature of things neither α_2 nor α_1 are very greatly affected by any such change. Even though the individual values in the survivor column in the life table in the range of ages from 20 to 45, for example, may be materially altered, this does not in general alter very greatly the mean age of this population group. A similar remark applies to the age group under 5 or under one and to the mean α of the net fertility curve. Even though this curve be changed quite considerably in absolute amplitude, this does not as a rule involve much change in the mean age of this portion of the curve.

The result of all this is that the exponent $(\alpha_2-\alpha_1)/\alpha$ is not very greatly affected by changes in the life table or in the net fertility curve. This is clearly brought out by the data exhibited in Table 2. It also accounts for the fact that the theoretical curve in Figs. 2 and 4, based on a general life table for the United States as a whole, very adequately fits the scatter of points relating to 45 States whose life tables differ materially, the expectation of life of white females at birth ranging from 52.2 to 66.8 years.

Effect of changing age limits of junior age group. The selection of the limits of the junior age group is in considerable measure arbitrary, although the resulting value of the replacement index is not unaffected by the choice. For reasons of expediency, the age group 0 to 5 will usually be employed. It is, however, interesting to ascertain within

[8] It should be noted that this fact arises from the choice of the age limits 20 to 45 in forming the replacement index. Had the limits 15 to 45 or 20 to 50 been used, the exponent would not be so nearly equal to unity.

what limits the values of J will vary if the limits of the junior age group are taken from 0 to 5, or to 4, 3, 2 or 1, or if the births are used instead of the junior age group. The variation in J produced by thus changing the age limits of the junior age group in a population with stable age distribution is exhibited in Table 3 on the basis of the life

TABLE 3

VARIATION IN REPLACEMENT INDEX WITH CHANGE OF UPPER AGE LIMIT OF JUNIOR AGE GROUP IN A POPULATION WITH STABLE AGE DISTRIBUTION, BASED ON LIFE TABLE FOR WHITE FEMALES, UNITED STATES, 1929–1931

Junior age group*	True Rate of Natural Increase per Head					
	.03640	.02800	.01736	.00265	−.02170	−.04516
	Replacement Index					
Age 0†	3.130	2.420	1.738	1.089	.490	.221
Under 1	3.052	2.373	1.717	1.087	.498	.228
Under 2	3.004	2.344	1.704	1.086	.503	.233
Under 3	2.955	2.315	1.690	1.084	.508	.237
Under 4	2.905	2.285	1.677	1.083	.513	.243
Under 5	2.856	2.255	1.663	1.082	.518	.248
30–35*	2.752			1.079		

* The senior age group in each case is 20–45 years, with the single exception of the case where the junior age group is 30–35 years, the senior age group here being taken as 50–75 years. The values shown in this table have been computed directly from the corresponding stable age distributions.
† Annual births (see text pp. 281–282).

table for the white female population of the United States in 1929–1931, and for a selected number of values of the true rate of natural increase. It will be seen that the variation, though not very marked, is not altogether negligible. Thus J, the replacement index, is not a unique measure of net fertility, and in this respect is inferior to the true rate of natural increase.

TABLE 4

Age groups	Mean Age			Ratio* $\frac{\alpha_2-\alpha_1}{\alpha}$
	Junior age group α_1	Senior age group α_2	Difference $\alpha_2-\alpha_1$	
Under 5 combined with 20 to 45	2.483	32.285	29.802	1.0630
30 to 35 combined with 50 to 75	32.491	61.125	28.634	1.0213

* α = mean age of net fertility curve.

Effect of simultaneous parallel shift of junior and senior age groups. Evidently the general character of the relations (27), (33) will be little affected by any change which leaves the exponent $(\alpha_2-\alpha_1)/\alpha$ approximately unaltered. Thus, if instead of the age groups "under 5" combined with "20 to 45," we base the computation on the age groups "30 to 35" combined with "50 to 75," we find the values shown in Table 4 for α_2 and α_1, their difference, and the exponent $(\alpha_2-\alpha_1)/\alpha$.

This suggests that from the same population we should be able to obtain a series of values of the replacement index J, by using successively higher pairs of equidistant age groups.

It should be noted that the replacement indexes derived from such successively higher pairs of age groups represent successively earlier generations. This gives us an opportunity in the case of an actual population, to obtain evidence of the time trend in the replacement index.

The computation has been carried out for a succession of pairs of age groups in the actual population of the United States at the time of the last census (1930). The results are shown in Table 5. The series of figures in column 3 very clearly exhibits the shift in the replacement index towards higher values, as we trace it backwards in time, indicating the higher fertility of early calendar years. The figures, however, for more remote years, can be taken only as a very rough measure.

TABLE 5

TREND OF REPLACEMENT INDEX FOR SUCCESSIVELY HIGHER AGE GROUPS, WHITE FEMALES IN THE U. S., IN 1930 COMPUTED ON THE BASIS OF A LIFE TABLE FOR WHITE FEMALES, U. S., 1929–1931

Junior age group (1)	Senior age group (2)	Replacement index J (3)	Average year of birth of junior group (4)
0– 5	20–45	1.088	1928
5–10	25–50	1.309	1923
10–15	30–55	1.359	1918
15–20	35–60	1.444	1913
20–25	40–65	1.558	1908
25–30	45–70	1.595	1903
30–35	50–75	1.700	1898
35–40	55–80	1.933	1893
40–45	60–85	1.886	1888
45–50	65–90	1.878	1883

It should also be noted that there is a fundamental difference between the replacement index computed on the basis of a junior age group concentrated near age zero, and a replacement index computed or more advanced age groups. In the former case the junior age group is affected but little by immigration, since relatively few small children enter the country. In the latter case the junior age group will be more and more influenced by immigration as the age limits selected for it advance. This fact must be taken into account in viewing the figures in the table. It will tend to raise the value of the replacement index computed for more advanced ages, because then the junior age group contains persons who are not children of the senior age group.

The senior age group will of course always be affected by immigration if the computation is based on the total population. This is as it should be if the replacement index for the total population is desired.

A separate index can, if desired, be computed for the native-born population alone.

Application to population with actual age distribution other than stable. Formulae have so far been developed only for a population with stable age distribution corresponding to the true rate of natural increase. From the example presented by the actual figures for the several States of the Union, as exhibited graphically in Figs. 1, 2, and 4, it is clear that certain of these formulae still apply even when the age distribution differs very materially from the stable, as shown in Fig. 3. This requires explanation, to which we now proceed.

Anticipating somewhat the argument and results that will be developed, it may briefly be stated that the applicability of the formulae to *any* population propagated essentially by the excess of births over deaths (i.e. in the absence of any considerable immigration of infants), arises from the fact that the zero ordinate (representing the births) in the age distribution diagram is always directly related to the area comprised within the reproductive ages, regardless of what may happen to the ordinates in other age regions of the diagram.

We now proceed to a more exact consideration of these relations.

Relation between total annual births in two calendar years one mean generation apart. Let $B(t)$ be the total annual births at time t, and let $m(a)$ denote the maternity frequency (gross fertility) of women of age a counting daughters only; let $p(a)$ be the probability for a newborn female to reach age a. Then, since the annual births $B(t)$ at time t arise from mothers born a years ago at time $(t-a)$, we have

$$\frac{\int_0^\infty B(t-a)p(a)m(a)da}{\int_0^\infty p(a)m(a)da} = \frac{B(t)}{\int_0^\infty p(a)m(a)da} \tag{34}$$

$$B(t-\bar{a}) = B(t)/R_0 \tag{35}$$

where $\bar{a}$ is the average age defined by the left-hand member of equation (34). If $B(t-a)$ is nearly linear as a function of a, within the limits of the reproductive age period, then the average $\bar{a}$ defined above is nearly the same as the average age[9] α defined by

$$\alpha \int_0^\infty p(a)m(a)da = \int_0^\infty ap(a)m(a)da \tag{36}$$

[9] This will be seen to result from the following demonstration that the average of a linear function of x is equal to the same function of the corresponding average of x.

If
$$\phi(x) = a + bx \tag{38a}$$

$$\bar{\phi}(x) = \frac{\int \phi(x)f(x)dx}{\int f(x)dx} = a + b\frac{\int xf(x)dx}{\int f(x)dx} = a + b\bar{x} \tag{38b}$$

that is, $\bar{a}$ is to this approximation simply the average age of the female net reproductivity curve, which has previously been denoted by α, and we can write

$$B(t)/B(t-\alpha) = R_0 \tag{37}$$

$$= e^{\rho\alpha} \text{ approximately.} \tag{38}$$

If $\alpha_2-\alpha_1$ does not differ greatly from α, it will still be nearly true that

$$B(t)/B(t-[\alpha_2-\alpha_1]) = e^{\rho(\alpha_2-\alpha_1)}. \tag{39}$$

Put

$$t = \tau - \alpha_1. \tag{40}$$

Then

$$B(\tau-\alpha_1)/B(\tau-\alpha_2) = e^{\rho(\alpha_2-\alpha_1)} \tag{41}$$

and since this is true for all values of τ, we can simply write

$$B(t-\alpha_1)/B(t-\alpha_2) = e^{\rho(\alpha_2-\alpha_1)}. \tag{42}$$

Now, for *any* population, J has been defined as

$$J = \frac{\int_p^q c(a)da}{\int_u^v c(a)da} \div \frac{\int_p^q p(a)da}{\int_u^v p(a)da} \tag{43}$$

and since the $N(t)c(a)da$ females of ages a to $a+da$ at time t are the survivors of the $B(t-a)da$ females born at time $t-a$ within a time interval da

$$N(t)c(a)da = B(t-a)p(a)da. \tag{44}$$

Hence

$$J = \frac{\int_p^q B(t-a)p(a)da}{\int_u^v B(t-a)p(a)da} \div \frac{\int_p^q p(a)da}{\int_u^v p(a)da} \tag{45}$$

$$= \frac{\int_p^q B(t-a)p(a)da}{\int_p^q p(a)da} \div \frac{\int_u^v B(t-a)p(a)da}{\int_u^v p(a)da} \tag{46}$$

$$= B(t-\alpha_1)/B(t-\alpha_2) \tag{47}$$

$$\log_e J = (\alpha_2-\alpha_1)\rho \quad \text{by (42).} \tag{48}$$

Lastly, since

$$\log_e R_0 = \rho\alpha \quad \text{(approximately)} \tag{49}$$

we have, in view of (48),

$$\log_e J = (\log_e R_0)(\alpha_2 - \alpha_1)/\alpha \tag{50}$$

$$J = R_0^{(\alpha_2-\alpha_1)/\alpha}. \tag{51}$$

These results explain why the relations (48) and (50), developed primarily for a population in stable age distribution, have been found to apply to an actual population with age distribution differing markedly from the stable. Also, since in the last step in the development above, all explicit reference to the life table cancels out, we have here the explanation of the fact that the curve computed on the basis of the life table for the United States as a whole adequately fits the scatter of points relating to 45 individual States having separate mortality characteristics.

Furthermore, the development leading up to equations (48) and (50) involves no assumptions regarding the age limits p, q of the junior age group, and u, v of the senior age group, except that the difference of their means should not be widely different from α, the mean age of the (female) net reproductivity curve. It follows that the conclusions reached, that is equations (48) and (50), continue to hold if the junior and senior age groups are shifted simultaneously so as to keep them the same distance apart. However, the particular value of J obtained relates in each case approximately to the mean calendar year of birth of the junior age group.

While the age limits p, q; u, v, are to some extent arbitrary, it is clear that the entire argument presupposes them to be so chosen that the persons comprised within the junior age group are *essentially* the children of those in the senior age group. This implies that the junior age group must not be unduly wide. In practice a width of five years has commonly been used, while the width of the senior age group has naturally been made to cover essentially the length of the (female) reproductive period.

Incidentally, it should be remarked that if there is any considerable immigration or emigration, the relation between the junior and senior age groups can in general[10] no longer be expressed exclusively in terms of the annual births within the United States; that is, equation (44) no longer holds. For this reason, in a population which in the past has had considerable immigration, the values of the replacement index J

[10] That is, when the junior age group is taken, not near age zero, but at some higher age.

computed from advancing pairs of age groups will tend to be unreliable, as has already been indicated (p. 286).

A property of the net reproductivity. The net reproductivity has been defined and computed as the ratio of the total births in two successive generations under the régime of given constant age schedules of fertility and mortality.

We have passed by without special mention an interesting property of the net reproductivity, expressed by equation (37) namely that this net reproductivity R_0 is also approximately the ratio of the total births, in any "closed"[11] population, at two epochs of time t and $(t-\alpha)$ taken α years apart, where α is the mean age of the (female) net reproductivity curve. In this case the R_0 so computed is not necessarily a constant, but relates to the fertility and mortality prevailing at the time t.

We have already made use of this property in the development of equation (48). We proceed now to another application.

Application to actual population of the United States. By way of a numerical example the relation

$$R_0 = B(t)/B(t - \alpha) \tag{37}$$

was applied to the population of the United States in 1928, for which a computation of R_0 was on record.[12] The total number of births in the Birth Registration Area in 1928 was 2,233,149; in 1929 it was 2,169,920. The corresponding fractions of the total population comprised within that area were 94.3 and 94.7 per cent respectively. Making due allowance for this, we find, for the mean total annual births 1928/1929 the figure of 2,330,000.

To apply our formula we must also have a figure for the total annual births 28.5 years prior to 1928/1929 that is, for the year 1900. Now, direct statistics of births in the United States are not available prior to 1915. But we have an estimate[13] of the birth rate in 1900, namely 29.48 per 1,000. The total population at that time was 75,994,575, from which we infer that the total births were 2,240,000. We thus have

$$B(t)/B(t - \alpha) = 2{,}330{,}000/2{,}240{,}000 \tag{52}$$

$$= 1.040 = R_0. \tag{53}$$

Actual determination from the fertility and mortality of the white females in the Birth Registration States of 1920 in 1928 gave $R_0 = 1.049$.[14]

[11] That is, a population growing solely by natural increase, in the exclusion of immigration or emigration.

[12] L. I. Dublin and A. J. Lotka, *Metron*, vol. 8, 1930, p. 113.

[13] L. I. Dublin and A. J. Lotka, *Journal of American Statistical Association*, 1925, vol. 20, p. 318.

[14] Id. *Metron*, 1930, vol. 8, p. 107.

A similar computation carried out for the United States in the year 1933 gives for R_0 the value .883 computed from the total births in 1933 and in 1905. The value of R_0 obtained directly from the birth statistics and estimated white female population of 1933 is .866.

Application to a population with logistic growth. While certain of the formulae developed in the preceding sections are of general application, it is interesting to observe how they work out when applied to the special case of a population growing according to the logistic curve.

It will be recalled that certain populations have followed this curve fairly closely over considerable periods of time. In particular, the population of the United States, until the date of the last census, was represented in very good approximation by the curve defined by the equation

$$N(t) = \frac{N_\infty}{1 + e^{-r_0 t'}} = \frac{197{,}493{,}000}{1 + e^{-.0314 t'}} \tag{54}$$

where t' is measured from 1914.6. At the time of the last census, the observed population differed by only three-tenths of one per cent from the value calculated according to that formula. Since then the rate of increase has probably been materially less than that demanded by the curve, and the extension of the curve into future years must be regarded as a mathematical construction rather than as in any sense a forecast. Nevertheless, in view of the close fit of the curve to the past growth of the population, and in view of the general interest of the curve, the case of a population growing according to this law makes an interesting example in terms of which to examine the applicability of formula (37) to the case of a population with age distribution other than stable.

On a previous occasion[15] the writer has developed the curve of annual births $B(t)$ at time t corresponding to the population $N(t)$ growing according to the logistic curve, on the basis of the constants characteristic of the population of the United States. The series of values thus obtained, while patterned essentially on the type of the American population, is to this extent hypothetical, that computations were conducted on the basis of a fixed life table (that of 1919–1920) and of a "closed" population, that is, one in which growth takes place entirely by an excess of births over deaths. Inasmuch as the purpose of the example here given is to illustrate the applicability of formulae (37) and (56) to a population with age distribution other than the "stable," the fact that the numerical data are hypothetical, to the extent indi-

[15] For details the reader must be referred to the original publication, A. J. Lotka, "Structure of a Growing Population," *Human Biology*, 1931, vol. 3, p. 459.

cated, is immaterial, and for the same reason we may, merely by way of example, base computations on that part of the curve which is entirely hypothetical and extends beyond the date of the last census. With this in mind, the formula (37) has been applied to the series of values of $B(t)$ computed on the basis indicated.[16] The results are shown

TABLE 6

COMPARISON OF RATIO OF ANNUAL BIRTHS AT TWO CALENDAR PERIODS ONE MEAN GENERATION APART, WITH CORRESPONDING RATIO OF TOTAL BIRTHS IN TWO SUCCESSIVE GENERATIONS; ALSO, CORRESPONDING VALUES OF THE TRUE RATE OF NATURAL INCREASE; COMPUTED FOR A LOGISTIC POPULATION WITH CHARACTERISTIC CONSTANTS OF THE UNITED STATES*

Year t	Annual births		Crude rate of natural increase, r§	Mean length of generation, T‡	Annual births
	$B(t)$	$B(t-\alpha)$†			$B(t-T)$
1	2	3	4	5	6
1800	218697	91897	.0306	27.78	93870
1825	454475	197055	.0296	27.80	201064
1850	890975	411709	.0277	27.84	419184
1875	1567415	815688	.0243	27.92	827261
1900	2330669	1462024	.0191	28.04	1474927
1920	2797246	2082021	.0143	28.15	2091788
1925	2882723	2231455	.0131	28.17	2240209
1950	3164261	2824622	.0077	28.30	2827609
1975	3303823	3136262	.0040	28.38	3137024
2000	3373610	3289582	.0020	28.43	3289754

Year t	Net reproductivity, $R_s(t)$ = ratio of total births in two successive generations			True rate of natural increase, $\rho(t)$, computed¶ from α, β, and $R_s(t)$, using values of $R_s(t)$ as given in		
	$\frac{B(t)R_s'}{\Sigma m'(a)c(a,t)}$§	$\frac{B(t)}{B(t-\alpha)}$	$\frac{B(t)}{B(t-T)}$	col. 7§	col. 8	col. 9
1	7	8	9	10	11	12
1800	2.329	2.380	2.330	.0304	.0312	.0304
1825	2.260	2.306	2.260	.0293	.0301	.0293
1850	2.127	2.164	2.125	.0271	.0277	.0271
1875	1.899	1.922	1.895	.0229	.0234	.0229
1900	1.588	1.594	1.580	.0165	.0166	.0163
1920	1.346	1.344	1.337	.0105	.0105	.0103
1925	1.296	1.292	1.287	.0092	.0091	.0089
1950	1.125	1.120	1.119	.0042	.0040	.0040
1975	1.056	1.053	1.053	.0019	.0018	.0018
2000	1.027	1.026	1.025	.0009	.0009	.0009

* Equation (54).

† $\alpha = 28.47$ years; $\beta = -45.39$.

‡ $T = \alpha + \frac{1}{2}\beta r$ (very nearly).

§ A. J. Lotka, "Structure of a Growing Population," *Human Biology*, 1931, vol. 3, p. 479; R_s' and $m'(a)$ are computed from observed mortality and fertility statistics in the United States, 1920, as explained *loc. cit.*, pp. 475–476.

¶ $\rho(t) = \frac{1}{\beta}\left\{-\alpha + \sqrt{\alpha^2 + 2\beta \log_e R_s(t)}\right\}$.

in column 8 of Table 6. For comparison the values of $R_0(t)$ and $\rho(t)$ computed directly from the age distribution in the logistic population at selected calendar periods are shown in columns (7) and (10) of the same table.

[16] *Loc. cit.* 459, 466.

It will be seen that the agreement is excellent for 1900 and later dates.

For the earlier dates a formula a little more exact than (37) can be given. It will be recalled that in the early stages of the logistic curve, growth is approximately according to the law of compound interest (geometric increase). In that case the mean length of the generation, which in preceding sections has been expressed (approximately) by α, can be expressed with greater accuracy by

$$T = \alpha + \tfrac{1}{2}\beta r_t \tag{55}$$

where r_t is the rate of natural increase at time t. This suggests replacing the formula (37) by

$$\frac{B(t)}{B(t-T)} = \frac{B(t)}{B(t-\alpha-\tfrac{1}{2}\beta r_t)} = R_0(t). \tag{56}$$

The results of the computation carried out on this basis are shown in columns (9) and (12) of Table 6. It will be seen that for calendar years up to and including 1875 the amended formula (56) gives excellent agreement with the values in columns (7) and (10) computed directly from the age distribution. For later calendar years unimportant divergences of one or two units in the last retained decimal appear.

SUMMARY

Part I presents numerical data of the computed values of the true rate of natural increase ρ, the net reproductivity R_0, and the replacement index J, for each of 45 States of the Union, and incidentally exhibits the relation between the net reproductivity and the degree of industrialization and urbanization of the several States. In this first part are also exhibited empirically, on the basis of the computed values, and illustrated by means of graphs, the relation between the replacement index and the true rate of natural increase, and also the relation between the replacement index and the net reproductivity.

Part II exhibits the rational development on a theoretical basis, of the relations between the replacement index, the net reproductivity, and the true rate of natural increase. The formulae are developed primarily on the basis of a population in stable age distribution. The numerical values of the various constants involved, and the effect of variations of these constants is discussed: Effect of changing age limits

of the junior age group; effect of simultaneous shift of junior and senior age groups.

Empirically, it has been found that the formulae developed on the basis of a stable age distribution were still applicable in close approximation to certain populations differing markedly from the stable age distribution. The reason for this continued applicability is investigated.

As a by-product of the investigation, it is found that the ratio R_0 between the total births in two successive generations, as computed from the fertility and mortality prevailing at a given instant, is also very nearly the ratio of the annual births in the same population at two calendar periods one mean generation apart, even though the fertility and mortality may in the meantime have undergone considerable changes. Examples are shown of the application of this to an actual population (United States, 1928 and 1900 and also 1933 and 1905) and to a theoretical population growing according to the logistic curve.

THE A.A.A., THE COTTON GROWERS, AND THE AGRICULTURAL PROBLEM*

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Bureau of Business Research, University of Texas

Cotton growing plays a very important part in our complicated economic and social organization in the United States. Correct understanding of the effects of the A.A.A. and the present administration of it on the welfare of the cotton industry and through it on our whole economic and social life can scarcely be attained without understanding the origin and extent of the influence of the cotton industry in shaping the nature and development of American agriculture and the relation of agriculture to modern trade and industry.

To bring these interdependent relationships of the cotton industry into proper perspective I propose in this paper to suggest a new approach to the cotton problem in particular and the whole agricultural problem in general. Accordingly, the first part of my discussion will be a brief analysis of the purpose and program of the A.A.A. and the effects of that program on cotton farmers and the cotton growing South down to the present time. The second part will discuss cotton as the key to agricultural recovery.

AGRICULTURAL PROBLEMS AS STATED BY THE A.A.A.

According to Title I of the A.A.A., the fundamental problem of agriculture as the framers of the Act conceived it was price disparity between agricultural commodities and other commodities. In the opinion of the framers of the A.A.A. the trouble lay in the relatively low prices of agricultural products due to over-production.

REMEDY PROPOSED

Briefly stated, the objective for cotton was price parity with an arbitrarily selected base period of July 1909 to August 1914. The program adopted to attain price parity for cotton was to levy a heavy processing tax on raw cotton consumption in the United States, to get money, to cut down acreage, to reduce supply, to raise price. Stripped of all its frills, this program is essentially an attempt to

* A paper presented at the Ninety-seventh Annual Meeting of the American Statistical Association, New York City, December 28, 1935.

I have chosen to discuss in this paper only the cotton growers and the agricultural problem. My colleague, Dr. F. A. Buechel, is making a study of the livestock industry similar to the one I am making for cotton.

bring about a redistribution of wealth by forcibly slowing down production, or the destruction of the processes of real wealth production in one branch of our economic organism, agriculture, to exact a monopoly advantage from others.

Undoubtedly, the belief in the feasibility of this idea was based on the discovery from an analysis of *Agricultural Yearbook* figures that random short crops of cotton in the United States have generally brought more dollars to the cotton growers than large crops coupled with the assumption that planned acreage reduction would bring short crops, and the further assumption that planned short crops would have the same results on prices as random short crops.

RESULTS OF THE COTTON RESTRICTION PROGRAM

To understand the far-reaching effects of the cotton restriction program it is necessary to know that the major portion of the economic life of the South revolves around cotton. Cotton is not only the main source of income for 2,000,000 cotton growers, almost a third of all the farmers in the United States, but either directly or indirectly the major activities of the cities and towns in the region revolve around it. The cotton business, then, is a great interdependent complex of activities in which each group engaged in it is greatly benefited by the efficient work of the others and is thus more or less obligated to them. Thus, the great mass of cotton choppers and pickers, ginners, cotton seed crushers, and cotton merchants in reality represent an enormous amount of more or less stand-by equipment to serve the cotton growers.

The cotton restriction program fails to recognize these interdependent relations. As a result, hundreds of thousands of people in the South have been forced on to relief rolls. Regardless of what visionary reformers may say, employment is still a function of production, and production in such a predominantly important enterprise as cotton production cannot be cut one-third without creating a grave unemployment problem.

Those with even a school boy understanding of the cotton industry are not surprised that long relief rolls persist in the South, when their attention is called to the fact that during the past three years cotton production has been cut approximately 13,400,000 bales. The total amount of effective employment destroyed by this ill-conceived policy, that the best way to get more is to do less, is staggering in the extreme. It has meant the loss of about $83,000,000 in cotton picking, $64,000,000 in cotton ginning, $47,000,000 in transportation, about $58,000,000 in compressing, warehousing, merchandising, and in other

marketing costs made up mostly of labor. The 13,400,000 bales of cotton the Government kept out of production would have resulted in the production of about 5,400,000 tons of cotton seed which would have had a manufacturing value of about $29,000,000. These items do not include preparation of land, planting, cultivation and hoeing, very important items in employment, and yet the figures I have given total $281,000,000. The predominant amount of this money would have gone to unskilled, unorganized laborers and tenant farmers, the very people who are now being forced to ask for a dole to keep from starving.

COTTON PRODUCTION CONTROL TO RAISE PRICE

Since the A.A.A. contends that price parity is the one big objective in agriculture and that the primary means of attaining that parity is restriction of production to raise price, in their opinion the measure of success must be the extent to which the price of cotton has been raised.

The season 1934–35 undoubtedly provided the most ideal opportunity for testing the value of the scheme. One of the most severe droughts in our history combined with Government-enforced acreage reduction reduced the United States crop to 9,469,000 gin bales, which was the lowest production since 1899 with the exception of the disastrous crop failure of 1921–22. In addition to the unprecedented short crop, the Government took out of the market about 4,500,000 bales of the crop with a 12-cent loan. Undoubtedly that, combined with private speculation, inaugurated one of the strongest holding movements in history. The combination of very low production, removal of one-third of this small crop from the market, and private speculation should have raised the price of cotton if such a program could do it. Did it do it then, or has it done it since? Let us see. If it has not, the futility of the program is obvious, and the injury to the farmers enormous.

COTTON PRICES IN GOLD

Cotton is a world commodity. Its price is a world price made in grains of gold. This is true because: (1) gold is a universal standard for measuring value; (2) cotton is a relatively non-perishable commodity in universal demand; and (3) American cotton has a merchandising force behind it offering it in all world markets. Since gold can be converted into any currency at that currency's gold value, changes in the gold price of cotton are caused by changes in supply and/or demand for cotton. It follows then that if there is an advance in the currency price in one country without a corresponding advance in the currency prices of other countries or the gold value of cotton, it is conclusive

proof that in the first instance the advance was due to a decline in the gold value of the currency. Let me illustrate: American cotton merchants with offices and close trading relations in all world cotton markets price cotton in the currency of each country in terms of its convertibility through exchange into grains of gold. Their objective is to sell their cotton in the market that will yield them the greatest number of grains of gold net. It is obvious, of course, that the sale yielding the greatest number of grains of gold net will also yield the greatest number of dollars or units of any other free currency. The amazing exactness with which this pricing is done is shown by the fact that trades are turned in the market on the basis of .0013 of a grain of gold per pound, equal to 1/100 of a cent at the present gold value of the dollar. This amounts to only one nickel a bale. How can it be done? It can be done because merchants deal in units of 100 bales in exporting cotton and selling to mills. Evidently these small trading margins show how entirely impossible it is for gold prices of cotton to get far out of line from one country to another and stay that way for long.

If the above statement is correct, then the price of cotton in United States dollars should have gone up approximately 69.5 per cent without any change in the world's gold price. On the other hand, if the price in dollars did go up about 69.5 per cent while the gold price remained the same, it is proof positive that it was the decline in the gold value of the dollar which put the price up in dollars, and the restriction program as a price-raising device has been a failure. Let us examine the facts.

The average price of American cotton in Havre, France, April, 1933, was 1.86 grains of gold per pound. In April, 1935, the average price was 1.95 grains of gold, an advance in the two years of .09 of a grain of gold. This is the equivalent of approximately 39 points, or .39 of a cent a pound. The average price of New Orleans spot cotton in April, 1933, was 6.88 cents. Taking into account the .09 of a grain of gold advance in the price in Havre, the average price of New Orleans spot cotton in April, 1935, should have been 12.05 cents in terms of the 13.7137 grains of gold or devalued dollar. It was actually only 11.80 cents. December 12, 1935, the price of American cotton in Havre was quoted at 1.86 grains of gold per pound. It is important to note that this was exactly the price in April, 1933. The price of New Orleans spot cotton, December 12, 1935, was 11.65 cents, and this is equal to 6.87 cents in terms of our old 23.22 grain dollar. On December 14, 1935, while this is being written, the price has declined to 11.47 cents, or only 6.77 cents in terms of the old dollar. These facts show beyond any shadow of a doubt that there has been no advance in the gold price of cotton and that the advance in the dollar price in the United States has been due

entirely to the reduction in the gold value of the dollar from 23.22 grains of gold to 13.7137 grains, or to 59.06 per cent of its former value. These facts also prove that the A. A. A. and the Bankhead Act as a means of raising the price of cotton have been utter failures.

Figure 1 shows the movement of the gold price of American cotton in Havre from 1931 to date, the New Orleans spot price in cents per

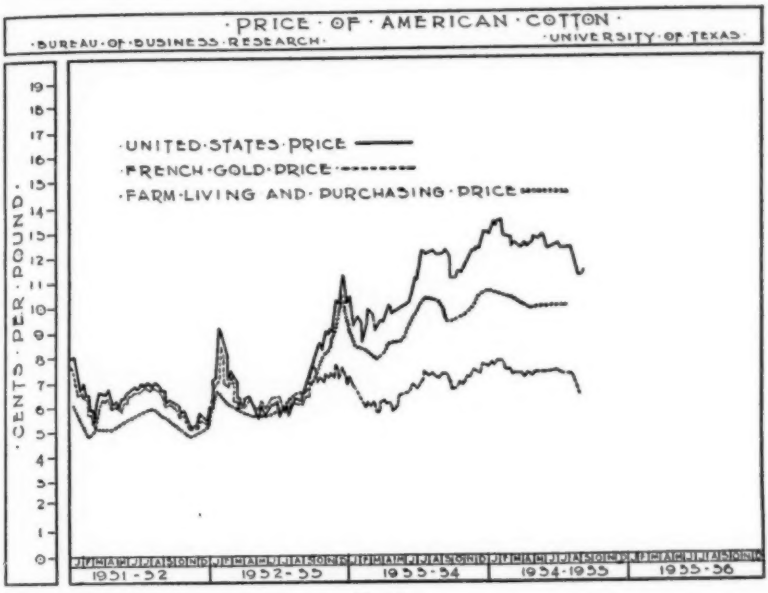

FIG. 1.

pound, and the farm living and purchasing price of cotton. This presents a more detailed analysis of the fact that gold prices of cotton have not advanced, that now, December 14, 1935, the price is actually lower than it was in April, 1933, and that the advance in the dollar price has been due to the cheapened dollar.

Figure 2 shows graphically the relation of the fall in the gold value of the dollar during the devaluation process and the dollar rise in the price of cotton.

The facts presented so far show that the cotton restriction program has prevented hundreds of millions of dollars in employment, has put millions of people on relief, and has been an utter failure as a scheme for raising cotton prices.

PARITY PRICE FOR COTTON

Since the passage of the A.A.A. much has been said about parity price. As defined in the Act and interpreted by the Administration,

parity price of cotton means a price with a buying power per pound of cotton equal to the average buying power of a pound from July 1909–August 1914. This parity price is worked out each month and published by the U. S. Department of Agriculture in the *Agricultural Situation*. For September the parity price is listed as 15.90 cents. Does that mean that if cotton growers were now getting 15.90 cents per pound they would have the same buying power as they had in 1909–14? Absolutely not. The fact is, if the method of raising prices is through reduction of

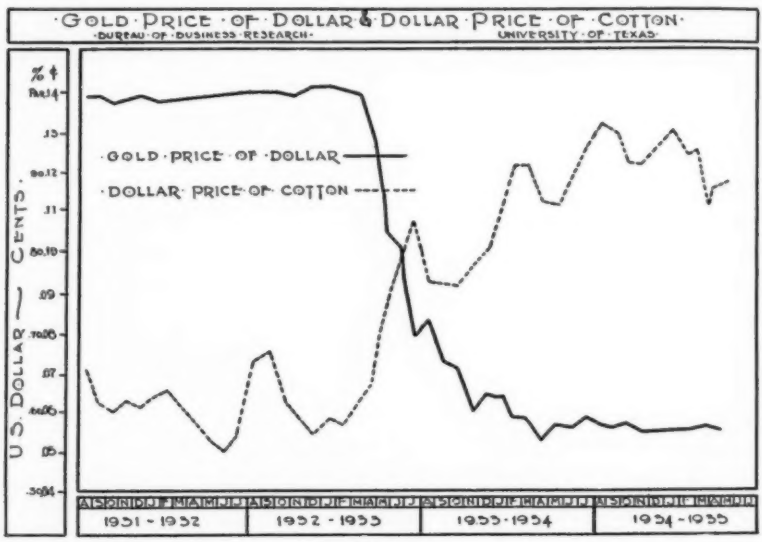
GOLD PRICE OF DOLLAR & DOLLAR PRICE OF COTTON
BUREAU OF BUSINESS RESEARCH
UNIVERSITY OF TEXAS
U.S. DOLLAR — CENTS
GOLD PRICE OF DOLLAR
DOLLAR PRICE OF COTTON
1931 - 1932
1932 - 1933
1933 - 1934
1934 - 1935

FIG. 2.

production, as the A.A.A. has attempted, the total income of the cotton grower may be lowered at the same time the price per pound is raised. Since the gold price of cotton has not been raised (reasons for which will be explained later) it takes no stretch of the imagination to see what would have happened in the South when cotton production was cut a third, as it has been, if we had not had the devaluation of the dollar at the same time which raised prices of cotton faster than most things the cotton growers buy. Indeed, the cotton growers would now be having the hardest time in their history; and, of course, thousands of the tenants and small farmers have had no profit from devaluation because they have been converted from producers to laborers. Their only alternative has been the relief roll. As prices of other things become adjusted to the cheap dollar, cotton will rapidly and surely lose the relative advantage in buying power it gained by devaluation. At that time cotton growers will pay and pay dearly for markets lost be-

cause of the A.A.A. and the Bankhead Act. Figure 1 shows that the "living and production" price of cotton is already rapidly leaving the dollar price and approaching the gold price. Make no mistake, that means that hard times for cotton growers.

Figure 3 shows the relationships between the New Orleans spot price, price actually received by farmers, the U. S. Department of Agriculture parity price, and, finally, what I have designated as parity income price.

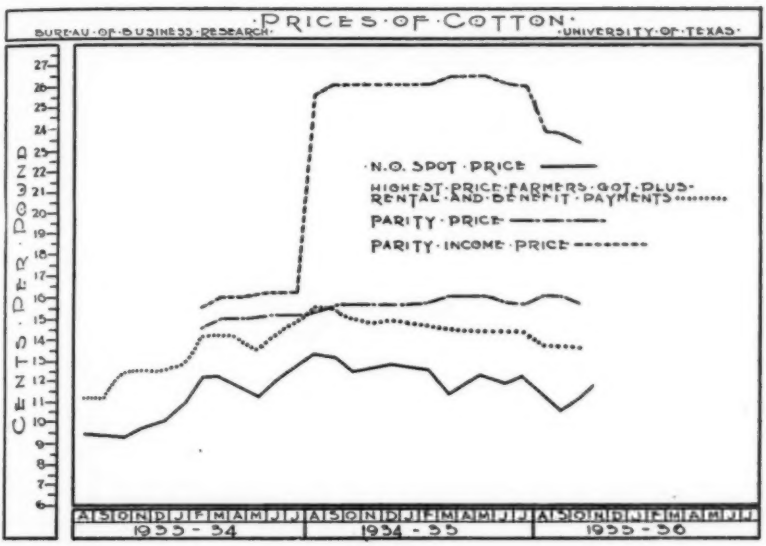
·PRICES·OF·COTTON·
BUREAU·OF·BUSINESS·RESEARCH·
·UNIVERSITY·OF·TEXAS·
CENTS·PER·POUND
·N.O. SPOT·PRICE
HIGHEST·PRICE·FARMERS·GOT·PLUS·RENTAL·AND·BENEFIT·PAYMENTS
PARITY·PRICE
PARITY·INCOME·PRICE
A S O N D J F M A M J J A S O N D J F M A M J J A S O N D J F M A M J J
1933 - 34
1934 - 35
1935 - 36

FIG. 3.

The New Orleans spot price, and the Government parity price need no further explanation. The price actually received by farmers is the actual price, the Government loan price or the guaranteed price whichever is higher, plus rental and benefits received by farmers converted to cents per pound on the basis of the amount of cotton produced.

Parity income price is a price times actual production which will give the cotton grower a total buying power equal to his average buying power for 1909–14. During the base period 1909–14 the average amount of cotton produced per cotton grower was about 8 bales. The average price was 12.4 cents per pound. This meant that the average cotton grower received $496 for his cotton. Parity price is now 15.90 cents, and parity income is therefore $636. The average farmer this year has grown only 5.1 bales. This means that each bale must bring $124.72 to yield $636 parity income, and to do that, the cotton grower must receive 24.94 cents per pound for his cotton. This year cotton

growers are receiving a guaranteed price of about 11.50 cents plus rental and benefit payments of about 2.00 cents on production, or a total of 13.50 cents which is only 54.1 per cent of parity income.

Doubtless some will question the validity of the parity income price. It will be argued that it costs the farmer less to produce 5 bales than 8, that he can use the released land for other crops not for sale. All that may be true, especially if one is considering only the landowner. On the other hand, it is absurd to base parity on anything other than income. This is especially true if the whole production complex is included such as the cotton growers, choppers, pickers, and ginners, as they should be. For fear that I shall be misunderstood as advocating parity income price for cotton growers equal to that of 1909–14, I wish to say that if the Government is going to presume to regulate and determine the relative incomes of different groups, I should like to protest the use of any fixed period as a base on the ground that it is unfair and illogical.

REASONS FOR FAILURE OF RESTRICTION PROGRAM

Aside from the fact that the restriction program typified by the A.A.A. and the Bankhead Act does not tackle the real problems of agriculture, as I shall discuss later, I wish at this juncture to point out three reasons for its immediate failure. They are: (1) rapid increase in foreign cotton production together with planned restriction of consumption of American cotton; (2) rapid increase in synthetic fiber production; and (3) impossibility of equitable administration of the control program.

Supplies of American and World Cotton. World supplies of American cotton have decreased more than 6,000,000 bales since 1932–33. Why is not that the cause of the advance in the dollar price of cotton? That is an important question and deserves careful analysis. Obviously, the answer is: because there has been no increase in the world gold price of cotton as shown by the gold price in Havre, Liverpool, and other world markets. Then are prices of cotton higher in gold in the United States than in foreign countries? No. If they were, no American cotton would be exported. Why have gold prices of American cotton not gone up? The explanation lies in a number of reasons. Only the major ones need be mentioned here. (1) While the supply of American cotton was being decreased by more than 6,000,000 bales, supplies of foreign cotton were being increased about 4,000,000 bales so that the net decline in supplies of all cotton has been less than 3,000,000 bales, and in spite of United States production this year being under 11,000,000 bales, world production is about 26,000,000 bales, or approximately what it was during the five years prior to the depression. American cotton

growers have thus reduced production about 13,400,000 bales in three years, but world supplies of all cotton were reduced about 2,800,000 bales only. Moreover, in spite of our drastic reduction, foreign production has been increased sufficiently to bring world annual production back to approaching normal. Has this been good business for American cotton growers? In 1933 the planners told us that foreign producers could not take our foreign markets; now they say that they would have taken them anyway. (2) Planned restriction of production of American cotton has been offset largely by planned restriction of consumption of American cotton. During the five pre-depression years, according to the figures of Garside of the New York Cotton Exchange, the average world consumption of American cotton was over 14,774,000 bales and all other cotton 9,986,000 bales. During the 1934–35 world consumption of American cotton was only 11,206,000 bales whereas world consumption of all other cotton was 14,222,000 bales. (3) The rapid increase in the production and consumption of foreign cotton is undoubtedly one of the greatest menaces to the advance in the gold price of cotton. Granting that the potential cotton production of the United States is still 15,000,000 bales or more, it is evident that world potential cotton production has been greatly increased since the inauguration of the control program. Recent figures from reliable sources show that foreign cotton production has been increased to more than 15,000,000 bales per annum. This means that world potential cotton production has been built up to over 30,000,000 bales, a figure far above normal consumption. (4) The program has created uncertainty which is one of the greatest deterrents to cotton consumption.

Figure 4 is a graphic presentation of figures published by John A. Todd of Liverpool, one of the best known authorities in the world on cotton crops. Todd's figures on foreign crops average about 600,000 bales above the figures of the U. S. Department of Agriculture. Todd's figures are used because United States figures for 1935–36 are not available. Special attention is called to the trend of production of American cotton as contrasted with foreign production.

Rise of Artificial Fibers. The rise in the production of artificial fibers undoubtedly offers the greatest menace to the cotton industry of the United States from a long-term viewpoint. It is a very serious fact and one that cotton growers should know that the world rayon and staple fiber production increased from 665,795,000 pounds in 1933 to 775,010,000 pounds in 1934. This increase is almost exactly equal to the annual increase in world cotton consumption over the past 25 years. It is highly important to realize that the greatest increases have occurred since the beginning of the cotton restriction program in 1933.

Figure 5 is a calculation I have made of the values of American and foreign crops of cotton in grains of gold. It is a striking illustration of the losses American cotton growers have sustained which the U.S. Government has forced them to hand over to foreign producers in the form of loss of markets. The chart is based on Todd's figures of production for each country, and the prices are averages calculated from

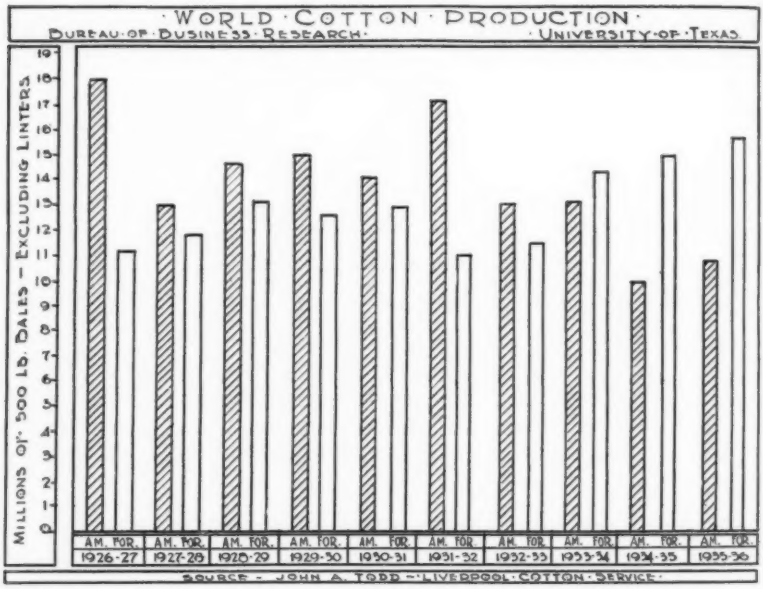

FIG. 4.

the Liverpool Weekly Circulars. Normally, the United States produced from 57 to 60 per cent of the world's cotton crops and cotton values. During the past three years, if we had gotten even 57 per cent of the value of the world's raw cotton production, the cotton growers of the South would have richer by about $600,000,000 than they are, not counting rental and benefit payments. Cotton growers have received $400,000,000 in rental and benefit payments and profit on cotton the Government sold them at below market price in 1933 to get them to plow up cotton. Stated in another way, this means that the United States is $600,000,000 poorer, the cotton growers themselves $200,000,000 poorer, and, what is more important, the cotton growers have lost a large market for their cotton.

How much has the cotton restriction program cost the cotton growers? No one will ever know exactly. As I have clearly shown already, it has been the important factor in causing us to lose foreign

market to the extent of about 3,000,000 bales annually. Moreover, consumption in the United States during the past year was down nearly 400,000 bales from the previous year. How much of this market will be permanently lost will depend on whether or not the Government sees fit to continue to restrict production and subsidize other fibers with a heavy processing tax on cotton.

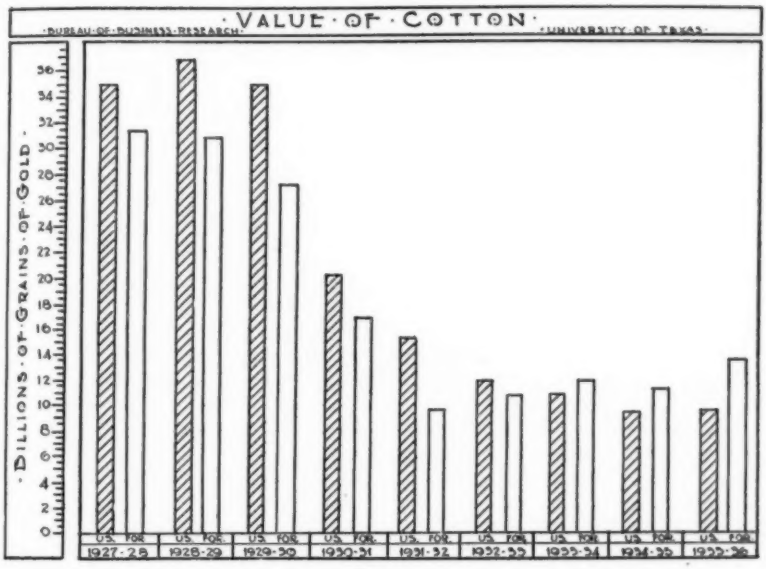

FIG. 5.

POSSIBILITY OF A RESTRICTION PROGRAM FOR THE FUTURE

What chance has the four-year cotton restriction program recently announced to raise prices and retrieve these losses? None. Before this do-less-and-get-more program was started the United States was producing from 57 to 60 per cent of the world's cotton. At that time a 30 per cent reduction in production in the United States accomplished a reduction of 18 per cent in world's cotton, a reduction of an additional 30 per cent in the United States would, at the same rate, reduce world production only 12 per cent. In the light of the facts already cited, is it not certain that another four years drastic restriction of cotton production, as already announced, will make even a 10,000,000-bale crop in the United States a surplus creating crop?

So much headline publicity has been given to the large sums of money turned loose by the Government to pay rental and benefit payments that most people think the cotton growers have been the recipients of unusual and especially copious bounties from the Federal

Government—that the cotton growers have been literally "paid off." No greater injustice could be done a people than to permit that erroneous impression to be perpetuated.

The point I wish to emphasize is that the cotton growers of the United States have been done an almost irreparable injury. The devaluation of the dollar gave the cotton growers a bonus of 69.5 per cent, and that would have been ample to have put them in a relatively prosperous condition, strengthened their hold on their foreign markets, and, in addition, made the whole cotton industry prosperous. Unfortunately, for the farmers and for the country as a whole, that opportunity was taken away by the cotton restriction program of the A.A.A. and the Bankhead Act. As a result, the South's ability to sustain itself largely on the cotton industry has received a serious blow from the control program, and only heavy bonuses for increased production can bring it gack to its former condition. Make no mistake, it will be a wise national policy to bring the South back at whatever cost because the burden cannot be localized in the South and the Southwest. It will be passed, in part, to other agricultural regions and to the industrial North and East in two important ways: first, in cutting off a large part of their market for their surplus product, and, second, in intense competition in production in both agriculture and industry. No amount of A.A.A., N.R.A., or other alphabetical restraints can stop it.

In view of the facts I have cited, will not history record the cotton restriction program of the United States as one of the greatest economic blunders ever committed by any people?

COTTON, THE KEY TO AGRICULTURAL AND INDUSTRIAL PROGRESS

The great strides in the United States which have been taken toward developing the highest standards of living of any country are a result of utilization of the country's abundant natural resources through division of labor in large scale production and regional specialization. Cotton has played the leading role in making possible specialized production on a large scale. Indeed, the development and growth of the cotton industry in the South gave direction to the economic development of the entire United States. The vehicle was the three-cornered trade centered around specialized production of cotton. In a very short space of time cotton became the world's greatest industrial raw material and commercial product and, as such, not only initiated regional specialization and large scale production in the United States but has been the most important factor in sustaining and developing specialized production to date. The South thus contributed the raw material

which played a leading part in the industrialization of Europe and New England and, with the cash received from raw cotton sold in these markets, bought the surplus food production of the Middle West. It was through this means that American agriculture was commercialized and permitted to develop on the basis of specialized regional production. Thus, our whole agricultural and industrial economy have been developed on the basis of individual and regional surplus production. Moreover, as has been pointed out by Mr. Elmer H. Johnson, my co-worker, the possibilities of adding to our standards of living by developing the possibilities of specialized regional production have been scarcely touched. The question now is, has the agricultural and industrial leadership in this country the vision to work out the necessary, smoothly coordinated, inter-regional relationships which will permit us to go ahead in this development?

The impasse confronting us now is not over-production about which there has been so much talk and little thought in recent months. There is not too much of cotton and cotton goods, wheat and bread, automobiles, electric refrigerators and other products, and never has been. The main problem, therefore, centers around the restoration of markets for regional surplus production. This involves two things: (1) the opening up of the inter-regional channels of trade at the place they are blocked, and (2) the restoration of equitable distribution of our national income as between regions and individuals.

Commercial cotton production not only paved the way for regional specialization of production in the United States, but the perpetuation of specialized cotton production or cash enterprise substitutes for it on an undiminished scale is essential to prevent the collapse of our system of regional production on its present scale. Certainly the easiest and surest way to prevent this threatened collapse is to restore foreign markets for American cotton to the extent of more than 50 per cent of our raw cotton production.

Do not be misled. This is not academic theory, and it is not strictly a cotton grower's problem. The extreme regional inter-dependence which has grown up makes it a national problem. The specialized wheat belt, the corn belt, the dairy regions, and the industrial centers in the North and East are in a very true sense as much or more interested in the restoration of cotton markets than the cotton growers themselves. To them it not only means a loss of a great market in the South but, in time, the development of formidable competitors.

Just how important is the export of raw cotton to our system of specialized regional production? Time will not permit a lengthy discussion of this question, and it is not necessary. Suffice it to say that

during the five pre-depression years the exports of cotton from the United States averaged about $775,000,000 and that does not include exports of linters and cotton seed, and cotton seed products. This large sum brought in from abroad served as an effective spring to start a tremendous flow of goods which furnished the means for marketing all regional surpluses throughout the country. When that large volume of buying power from abroad is cut off and no adequate substitute is provided, the South will be forced into other types of agriculture and into more manufacturing, and no amount of repressive measures can stop it.

In view of these facts it is clear that the agricultural problems of the United States will not be solved until the cotton problem is solved, and cotton production restriction will not solve the cotton problem but will greatly complicate the problems of agriculture by further disrupting specialized regional production.

DISCUSSION

Public discussions of the Agricultural Adjustment activities of 1933–1935 are useful in so far as they contribute to a clearer understanding of the major problems involved. It is doubtful if clear understanding can be promoted unless we first recognize the emergency situation of 1932–33 and the practical measures that the situation demanded. A second requirement is a clear understanding of the A.A.A. objectives. A third requirement is an appreciation of the complex interrelations between prices, supplies, consumption, domestic and foreign demand with conclusions drawn from them that are the result of correct statistical and economic analyses. A fourth is adequate facts rather than guesses as to the effects on farmers and other groups; and a fifth requirement, perhaps more important than all the rest, is an unbiased attitude. Dr. Cox's paper on "The A.A.A., the Cotton Growers, and the Agricultural Problem" fails to meet these requirements.

Dr. Cox approaches his subject with an unmistakably biased unscientific attitude. I need only quote from the mimeographed copy of his paper which was distributed widely at the time it was delivered. In discussing the regional aspects of the agricultural programs, he writes:

"It (the A.A.A.) advises the cotton grower to use his surplus land to grow his own food and feed crops, the dairyman to grow his own feed, the corn belt feeder to put his surplus corn acreage into pasture, to grow his own feeder steers, and the ranchman to—well, since he did not see fit to join the program—to go to hell."

His principal theme is that the A.A.A. has been an utter failure since the world gold price of cotton in December 1935 was about the same as in April 1933. In this part of his argument, Dr. Cox not only misreads the price objective of the Agricultural Adjustment Act but fails to appreciate that gold prices for basic commodities have been depressed by the world-wide devaluation of currencies.

The purpose of the A.A.A. was not to raise world prices but to give farmers parity returns on domestic consumption and to let the balance of their production go at world prices. The higher price for domestically consumed production was to be given to producers in the form of benefit payments. In the case of cotton, the price objective was actually attained. The farm price of cotton in 1934 including the price equivalent of benefit payments was 9 per cent greater than the parity price and in 1935 it was 5 per cent greater.[1]

Dr. Cox also misreads the objectives of the A.A.A. in his references to planned curtailment of consumption. Consumption according to the Act was to be taxed, and processing taxes were not to be so large as to curtail consumption or cause stocks to pile up. As a matter of fact, the real consumption objective was to increase consumption by curtailing an excessive carryover, raising domestic prices and stimulating general business.

Like many others who have attempted to damn the A.A.A. by showing that devaluation of the dollar was the chief factor in raising domestic cotton prices, Dr. Cox stops short in his gold price analyses without observing two important dilemmas. One dilemma is created by his argument that 13.4 million bales of cotton have been kept out of production by the curtailment programs. He does not raise the question, how much lower would cotton prices be now if that much cotton, equal to one full crop, were added to the present supply? Statistical analyses of cotton prices, such as Dr. Cox does not resort to, show that the addition of one million bales of American cotton to the carryover lowers prices by $\frac{3}{4}$ to 1 cent per pound. Assuming that lower prices would have prevailed during 1933–35 without the control programs, possibly 5 million bales of the additional 13.4 million might have been consumed. This would leave about 8 million bales to be added to the carryover of the present season, making a world carryover of American cotton of 18 million bales or four to five times normal—and cotton prices would probably be about 6 cents lower. The fact of the matter is of course that neither Dr. Cox nor anyone else knows how much cotton has been kept out of production, for no one knows what acreage reductions farmers would have made had the 17 million bale crop of 1933 been allowed to come onto the spot markets.

The other dilemma arises from the argument that the world gold price of cotton failed to rise in the face of curtailed production because of the expansion of foreign production and the "planned restriction of consumption of American cotton." If the world gold price has not been raised, whence comes the stimulus to foreign production? This problem suggests that the gold price does not now have the significance that Dr. Cox thinks it has and that the increase in foreign production is due to other factors that he has not taken into account. It is not the gold price of cotton but the currency price received by our farmers as compared with prices of other products that influences acreage, and it is not the gold price but the currency price

[1] See report on *Facts Relating to Cotton Prices and Purchasing Power* by L. H. Bean, A. A. A., August, 1935.

of cotton goods relative to prices of other products that influences consumption. This is true of the foreign cotton producing and consuming countries as well.

Those who, like Dr. Cox, dismiss the effect of the A.A.A. on cotton prices by referring to the low gold price fail to observe that cotton is not the only world commodity the price of which on a gold basis has remained practically unchanged. For example, the world gold price of wheat in Great Britain was only 6 cents higher in December 1935 than in April 1933, whereas the currency price was 41 cents higher. Dr. Cox cannot argue in this case that the reduction in the U. S. has been offset by an increase in foreign production, as he does with cotton. In fact, wheat production outside the U. S. was also reduced by weather and the excess in carryover has been reduced from 500 million bushels to nothing. Such failures as these of gold prices to rise in the face of reduced supplies and increased demand are clear indications that the general world-wide devaluation of currencies has altered the significance of gold as a measure of value. The problem is more complex than Dr. Cox indicates.

Foreign production has expanded but the reason for it is not predominantly the A.A.A. cotton program. This is revealed even by Dr. Cox's inadequate data and analysis. Dr. Cox presents an illustration showing an increase of 4 million bales of foreign production between the 1932–33 and the 1935–36 seasons. He does not point out that nearly 3 million bales of that increase occurred between the 1932–33 and the 1933–34 seasons, before the A.A.A. program adopted in the summer of 1933 was announced. The 1933–34 foreign acreage was the result of price conditions of the previous season and of other preceding factors. According to Dr. Cox's data, only about one-fourth of the increased foreign production he attributes to the A.A.A. occurred during the two years when the U. S. programs could have been responsible.

As a matter of fact, had Dr. Cox extended his illustration back to 1920 or earlier, it would have been clearer than in his chart from 1926 to 1935 that the recent increase in foreign production is only a continuation of a long-time persistent upward trend. One needs only project the prewar rate of expansion to see that without any A.A.A. programs, foreign production would have attained the volume that was actually produced in 1935. Another proof of this contention is contained in statistical analyses of foreign cotton acreage which enable us to determine how much of the increase in acreage is due to the price of cotton and how much to all other factors, such as the relatively low prices of coffee in Brazil and of grains and other competing crops in India and Egypt. We find that foreign cotton acreage expanded from 1921 to 1925 for reasons other than cotton prices, and that another period of expansion due to factors other than cotton prices began immediately with the onset of the depression after 1929. Had cotton prices remained unchanged during the last six years and had there been no A.A.A. programs, foreign acreage would have increased about as much as it actually did. Less than half of the increase in foreign acreage since 1933 can be

attributed to the higher currency price of American cotton and part of that higher currency price is due to other factors besides the A.A.A. programs.

Dr. Cox suggests that it should have been obvious from the beginning that foreigners would progressively increase production as the U. S. undertook to keep down supply. He fails, however, to recognize and to comment on the inevitability of that expansion regardless of what we did and to appreciate the necessity of helping the American cotton farmer and most of the other economic groups of the South cushion the shock of that expansion.

The other fact given by Dr. Cox for the A.A.A. failure to affect cotton prices, the rapid increase in synthetic fiber production, is even more inaccurate. He calls attention to "the serious fact" that world rayon and staple fiber production increased from 666 million pounds in 1933 to 775 million pounds in 1934 and then adds this entirely incorrect statement, "It is highly important to realize that the greatest increases have occurred since the beginning of the cotton restriction program in 1933." The increase between 1933 and 1934 was 109 million pounds but the previous increase, before the A.A.A. could have affected rayon production, was 139 million pounds. Furthermore, the increase between 1933 and 1934 was 16 per cent and the previous increase was considerably greater—26 per cent. The average annual increases in 1927, 1928 and 1929 of 28 per cent and in 1923, 1924 and 1925 of 35 per cent were also greater than the 1934 increase of 16 per cent.

Dr. Cox looks upon the cotton problem chiefly in terms of volume, and fails to see that there is a mid-point between abundant production that periodically about wrecks the South and unwise scarcity programs. The A.A.A. programs aimed at supplies more nearly in balance with demand conditions and eschewed the extremes of unwise scarcity and ruinous surpluses. A temporary curtailment of production to bring down a staggering carryover that kept prices low and business in the South close to a standstill in 1932–33, he considers is destruction of the processes of real wealth production. Over and over again the South, during the fifty years before the A.A.A., created "real wealth" in the form of bumper crops, and almost invariably these bumper crops gave the South and particularly the producers temporary economic set-backs. Actually, cotton farmers seldom realized "real wealth" from their bumper crops, the bulk of which went abroad at bargain prices or clogged up the economic machinery of the South in such a way as caused bank failures, increased indebtedness, and a perpetuation of low living standards. In 1926 cotton farmers produced "real wealth" in Dr. Cox's terms amounting to 18 million bales, a record crop, but received a gross return of about 500 million dollars less than in the previous year, and the cash left over after out-of-pocket expenses gave the average cotton producer a per capita purchasing power of only two-thirds of that of 1925 and two-thirds of his prewar standard of living. In 1931 the South again produced a large crop of 17 million bales. This time there was practically nothing left over after out-of-pocket expenses and the purchasing power per capita went down to about 12 per cent of what it had been in 1925 and before the war.

After this sort of "real wealth" production, the South has invariably curtailed its production so as to restore economic life in the South. In 1927 they reduced their acreage by 15 per cent, which together with a reduction in yields per acre brought about a larger gross and net income. All the other economic groups in the South profited from that improvement in values brought about by a curtailment in volume. Even those who depend on volume found they could move the large carryover from the 1926 crop much more freely at the higher prices of 1927–28 than at the low prices of 1926–27.

Similarly, in 1932 after the so-called real wealth of the large crop of 1931, farmers reduced their acreage and with reduced yields per acre only 13 million bales were produced. Even at the bottom of the depression in 1932 the income from the crop after out-of-pocket expenses was greater than the balance available from the large crop of 1931. On a small scale, in an unorganized way, farmers demonstrated in 1932 the wisdom of restraining production to permit the enormous carryover to come down to normal proportions. The prospect of a reduction in carryover as a result of the smaller crop and the resulting lift in cotton prices that the smaller 1932 crop brought about during the last half of 1932, were chiefly responsible for the temporary improvement in industrial activity after July 1932. Cotton consumption increased as prices were stimulated by the restrained production; 260,000 people were put back to work in the textile industry between July and October 1932, and weekly pay rolls were increased 7.5 million dollars. This was an increase of 35 per cent in employment and 64 per cent in pay rolls. In that brief period when the A.A.A. programs were being conceived, farmers themselves had demonstrated the nature of the practical approach to the cotton problem in the emergency of 1932–33, that was to be put into operation more effectively under Federal stimulus in 1933–35 to help bring about economic revival in the South and other parts of the country. The majority of farmers and the business interests of the South and other parts of the country dependent on the South are aware of the economic activity that has sprung up since 1932, even though like economists, statisticians, politicians and others, they are unable to appraise the separate influences on that revival that should be attributed to A.A.A., monetary devaluation, or other Federal as well as private activities.

Dr. Cox's prejudiced, loose and inaccurate references to the A.A.A. contribution to the unemployment and relief rolls of the South add more confusion than light. In one part of his paper he writes: "Hundreds of thousands of people in the South have been forced on to relief rolls as a result of production restriction"; in another, he writes that it "has put millions of people on relief." It is of course true that the A.A.A. dealt chiefly with production, but it attempted to prevent dismissal of tenants and share croppers by coöperating farmers, and it banked on the benefits to producers being generally distributed through the processes of trade, commerce and industry so that all groups of the South would share in economic revival. To what extent that was accomplished, cannot be determined. Dr. Cox, it is clear,

does not have any comprehensive data on displacement from farms and contributions to unemployment and relief, nor does anyone else. In a recent report by the Bureau of Agricultural Economics on this question, at the request of the Secretary of Agriculture, the Bureau was unable to draw any general conclusions because a basis for such conclusions does not exist. Conclusions based on inadequate samples may serve journalistic purposes but they do not add to balanced understanding.

There are two sources of data that throw only partial light on this question of increased relief rolls and tenant displacement, the Federal Emergency Relief Administration and the Bureau of the Census. According to the reports of the F.E.R.A. the number of people on relief in August 1933 constituted 12 per cent of the population. The relief load was also 12 per cent in Texas, but 14 per cent in Mississippi, 15 per cent in Louisiana, 18 per cent in Arkansas and Alabama, 20 per cent in South Carolina and 27 per cent in Oklahoma. This was the situation before the A.A.A. programs of 1933, 1934 and 1935 could have had any effect on the relief situation. In August 1935 the relief load for the country as a whole, after increasing during 1934, was again 12 per cent of the total population but it was relatively lighter in the cotton producing states than in 1933. In the two-year period the percentage for Texas, where Dr. Cox says the relief problem is still a staggering one, fell from 12 to 10; for Mississippi from 14 to 8; for Louisiana from 15 to 10; in Arkansas from 18 to 11; in Alabama from 18 to 10; in South Carolina from 20 to 8; and in Oklahoma from 27 to 19.

The 1935 census data reveal some striking changes in tenure between 1930 and 1935, but it is impossible to determine from that source alone how much of these changes took place between 1929 and 1933 and how much during 1933–35. For ten of the cotton states there was a reduction of about 31,000 in the number of white croppers, but an increase of about 106,000 in white tenants other than croppers or a net increase of 75,000 in white tenants. In addition, there was an increase of about 96,000 in the number of white owner-operators. Among the colored farmers, there was a decrease in croppers amounting to about 22,000 and in tenants other than croppers of about 47,000 or a total decrease of 69,000. Unbiased students of the South believe that a very large part of this shift among the colored tenants took place during the severest years of this depression, 1931 and 1932, when the cotton crops returned practically nothing after paying out-of-pocket costs. For the South as a whole, there were about 160,000 more farm owner-operators in 1935 than in 1930 and about 40,000 more tenants.

Dr. Cox's estimates of losses to other groups due to reduced volume are even less reliable than his estimate of the total reduction in production upon which they are apparently based. In the first place, the volume of business "lost" cannot be determined unless we know what farmers would have done without the 1933–34–35 programs. Secondly, he does not take into account losses that certainly would have resulted if carryover had been allowed to pile up even higher and perpetuate a low price structure in the South for cotton, for land, for wages and other services.

Dr. Cox has attempted to do in his paper "The A.A.A., the Cotton Growers, and the Agricultural Problem" what unbiased scholars and technically trained statisticians and economists are going to do more successfully during the next few years. After 1936 there will probably be a much smaller market for hasty and exaggerated criticism of the A.A.A. cotton program, just as there will be little need for exaggerated claims for its beneficial effects.

L. H. BEAN

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REJOINDER

Mr. Bean's opinion of me neither answers my arguments nor lessens the force of the facts presented. He involves himself in a maze of undigested figures and then says: "It (the cotton problem) is more complex than Dr. Cox indicates." My reply is that often-times a thing seems complicated to one merely because he does not understand it.

Limited space allotted permits me to deal only with major issues.

The main Theses in my paper are:

(1) That parity price with 1909–1914, which is the main objective of the A.A.A., is unsound and that parity income should be the objective.
(2) That the gold price of cotton has not advanced, and that dollar prices advanced almost exactly in proportion to the decline in the gold value of the dollar.
(3) That a restriction program to raise price to raise farmers income will not work in the case of cotton because planned restriction of production is met by (a) planned restriction of consumption of American cotton, (b) planned expansion of production abroad, and (c) increased competition of substitutes.
(4) That cotton production restriction has put hundreds of thousands, even millions, on relief.
(5) That specialized cotton production occupies a key position in our system of specialized, large scale, regional production, and that it is of extreme national importance to restore foreign markets for American cotton.

(1) *Parity Price and Parity Income.* In his attempt to dodge the force of the facts I presented on parity prices and income Mr. Bean did the very unusual thing of making an interpretation of the purposes of the A.A.A. wholly unwarranted by the law and at variance with its universal interpretation. He said the purpose was "to give farmers parity returns on domestic consumption and let the balance of their production go at world prices." Does Mr. Bean realize what he has said? If this was the purpose of the law, the Department of Agriculture misled the farmers and Congress is grossly ignorant of what it had done.

Let us analyze Mr. Bean's statement into figures. Parity return on domestic consumption cannot mean anything other than parity price times

domestic consumption. During 1934–1935, e.g. domestic consumption was 5,360,000 bales. Thus the domestic allotment would be about 2½ bales on the average. Parity price times this allotment would be approximately $200. The remaining 2.6 bales of the average production per farmer in 1934–1935 brought the farmer about $150 a total income justified under A.A.A. from lint cotton of about $350, according to Mr. Bean. If he bases parity consumption on the period 1909–1914 instead of 1934–1935 it would be even less. Remember the cotton grower got an average of about $496 annually out of lint cotton during 1909–1914. I am sure few Congressmen and no cotton growers would agree with Mr. Bean. To show that I have not misinterpreted his statement note this. He said: "In the case of cotton, the price objective was actually attained. The farm price of cotton in 1934 including the price equivalent of benefit payments was 9 per cent greater than parity price." Now if you apply all rental and benefit payments in price equivalents to domestic consumption alone you get a price approximately 9 per cent above parity price.

(2) *The Gold Price of Cotton.* Mr. Bean does not deny the validity of my thesis Number 2. He tries to establish an alibi by saying prices would have gone much lower had it not been for the A.A.A. At this point he presents what he calls two dilemmas. To present the first he asks, how much lower prices would have gone had we produced the extra 13.4 million bales. His answer is 6 cents which would make cotton worth now 5 cents in our "streamlined dollar" or less than 3 cents in terms of the old dollar. My answer is, I do not know. My guess is we would not have produced that many extra bales, foreigners would have produced less, there would have been fewer substitutes, some increased consumption, and the price slightly lower.

The second dilemma is "if world gold price has not been raised, whence come stimulus to foreign production?" My answer is, we virtually gave foreigners a guaranteed price and market by our restriction program and Government loans above market price; depreciated currencies in cotton growing countries helped, and United States tariff barriers, bi-lateral trade agreements along with other things, played a part.

(3) *Restriction Program.* Reasons for failure of planned scarcity program. I argued; (a) The theory of planned scarcity is based on the false premise that planned short crops and random short crops are comparable. Mr. Bean did not get the point. Planned restriction of production of cotton in the United States was met by planned restriction of consumption of American cotton. Mr. Bean's reply is that the A.A.A. did not plan decreased consumption. No, but the spinners of the world did. In this connection he makes the naïve statement that the A.A.A. planned increased consumption by using the processing tax to curtail excessive carryover, raise domestic prices, and stimulate general business. It was a wonderful plan but as the following figures show it did not work. During the year 1934–1935 United States cotton consumption was down 340,000 bales from the previous year and 777,000 bales from the year before even though we were supposed to be coming out of the depression. Foreign consumption of American cotton was

even worse. It dropped from 7,981,000 in 1933–1934, to 6,098,000 bales in 1934–1935. A slight miscarriage of plans I would say.

Mr. Bean says cotton consumption rose after July, 1932, because the farmers curtailed production at the same time the brilliant idea of "*do less to get more*" was being conceived. In the first place, in July it is impossible to tell what the new crop will be, and in the second place to imply that vague ideas of A.A.A. in the minds of a few people detached from business and ten months before enactment should have such potency is putting it rather strongly to say the least. Mr. Bean's argument at this point is really a marvelous piece of mental gymnastics. It shows what one with a fertile imagination, few first-hand facts, and no understanding of the economic processes he is discussing can do. It is a shame to explode his theory. Nevertheless, it must be done. The fact is the foundation for the revival of cotton manufacturing Mr. Bean speaks of was laid in the low prices of cotton in the winter of 1931 and the spring of 1932. About this I am not guessing; throughout the whole period I gathered from merchants every fifteen days their sales of cotton for delivery in forward month both to domestic mills and to export. By July, 1932, domestic mills and foreign importers had bought more cotton for summer and early fall delivery than for any similar period since 1926, another period very similar. This favorable situation in the cotton industry laid on the foundation of cheap cotton coincided with other factors favorable to the revival of business both in this country and abroad.

(b) Planned expansion of production abroad. Mr. Bean's only answer is what I said it would be "foreigners would have done it anyway," even though in 1933 it was said they could not do it. He said I did not go far enough back with my statistics on foreign production. He suggested 1920, so, to 1920 we go. The straight line trend of foreign production based on the period 1920 through 1932–1933 or down to the period under discussion shows that foreign production for 1935–1936 should have been 13,170,000 bales; it was actually over 15,000,000. Trends in United States production over the same period show United States production in 1935–1936 should have been 17,065,000 bales; it was 10,417,000. The trends from 1890 show the same relative growth. The only way I can imagine that Mr. Bean came to the conclusion he did was that he did the unscientific thing of including the last three years in his calculations when the United States farmers were "tied down" and our Government gave foreign producers the equivalent of a guaranteed price.

(c) Increased competition of substitutes. Mr. Bean says I am inaccurate and incorrect in my analysis. He bases his indictment on a misstatement of fact and the use of very false statistical practice. There was, as he points out, a 139 million pounds increase in rayon production in 1933 over 1932. His error was in thinking that was prior to the restriction program. The fact is, of course, the increase occurred in 1933 and that was the year the cotton processing tax was put on and the "cotton plow-up" occurred so that it was very much a year of restriction. In no year prior to 1933 did increased production reach 100 million pounds.

Those who know Mr. Bean will be surprised to find him presenting an argument based on percentage comparisons where each percentage is calculated on a different base. For example, he points out that the percentage increase in rayon production in 1925 over 1924 was 35 per cent, as against 16 per cent increase in 1934 over 1933. Now, the 35 per cent increase in 1925 over 1924 represented only 15.6 million pounds, whereas the 16 per cent in 1935 represented 109 million pounds. Surely Mr. Bean knows that this sort of comparison is incorrect, and yet there it is.

(4) *About Relief.* Mr. Bean characterizes my statements as "prejudiced, loose, and inaccurate." Unfortunately, at that time I had to talk in general terms as to numbers on relief and please note I used general expressions to make that plain. It seems the Government has been rather hesitant in gathering and publishing data on the number of unemployed, the number receiving Government relief, and why. I have been told that the Bureau of Agricultural Economics made a study of the A.A.A., and among other things its effects on employment in cotton production and came to the conclusion that there were 983,000 fewer men used in the production of the 1934–1935 crop than on an average during 1928–1932. Unfortunately, that figure was later censored out of the report by the A.A.A., and yet Mr. Bean says I am prejudiced and blames me for not giving exact figures. He says the number on relief has been decreased. Yes, nominally but how? Because the great mass of unemployed were ordered taken off relief and put on so called work projects.

A. B. Cox

ERRORS OF THE SECOND KIND IN TESTING "STUDENT'S" HYPOTHESIS

By J. Neyman and B. Tokarska

Consider a normally distributed variate x having the population mean, a, and the population standard error, σ. The probability law for x will, therefore be

$$(1) \qquad p(x) = \frac{1}{\sigma\sqrt{2\pi}} e^{-(x-a)^2/2\sigma^2}.$$

Assume that certain observations are able to provide us with a single value of the variate x and also with the estimate of σ^2, say s^2 independent of x and based on n degrees of freedom. Suppose further that we do not have any *a priori* information about the values of either a or σ but that we wish to test a hypothesis, say H_0 that $a = a_0$ while the value of σ is more or less indifferent to us. The hypothesis H_0 thus stated is called "Student's" hypothesis.[1]

It will be useful to distinguish two special forms of "Student's" hypothesis.

(1) *Symmetrical Form.* Here we ask simply whether $a = a_0$ and should like to detect any difference between the true value of a and the hypothetical one, a_0, whether $a < a_0$ or $a_0 < a$. In this case the hypothesis tested, say H_s, assumes that $a = a_0$ while the set of admissible alternatives contains any hypothesis ascribing to a a value other than a_0.

(2) *Asymmetrical Form.* The question asked here may be either (a) Is the true value of $a \leqq a_0$? or (b) Is the true value of $a \geqq a_0$? The cases (a) and (b) are clearly analogous and therefore we shall consider more closely only the first. Mathematically the problem is reduced to that of testing a hypothesis, say H_a, assuming that $a \leqq a_0$ while the set of admissible alternatives contains only such hypotheses which ascribe to a the values $a > a_0$.

In practical problems it is the asymmetrical form that is most frequently met with. This may be illustrated by the following examples.

Example I. A large brewery recommends to farmers a special variety of barley, say variety V_0. A breeder recommends a certain new variety, say V_1, which in his opinion is able to give a larger average value of a character X desirable for the brewery. The brewery may like to carry out experiments to test the advantages of the two varieties V_0 and V_1. Denote by ξ_0 and ξ_1 the population means of the character X

[1] "Student": "On the Probable Error of the Mean," *Biometrika*, Vol. VI, 1908, pp. 1–25.

corresponding to the varieties V_0 and V_1 respectively and by a the difference

$$a = \xi_1 - \xi_0. \tag{2}$$

If $a \leqq 0$ i.e. if $\xi_1 \leqq \xi_0$ then there will be no reason for the brewery for substituting the new variety V_1 instead of the established standard V_0. On the other hand, if the experiment provides sufficient evidence that actually $a > 0$ i.e. that $\xi_0 < \xi_1$ it may be worth while to recommend the variety V_1 instead of V_0. The hypothesis to test is therefore that $a \leqq 0$ while the alternatives assume that $a > 0$.

Example II. A buyer agrees to accept a large consignment of electric lamps at a fixed price on the assumption that the average length of life of the lamps, say a, exceeds a certain limit a_0. To test this circumstance a sample of several lamps is analysed and it is seen that the question asked is reduced to the test of an asymmetrical hypothesis, namely that $a \leqq a_0$ the alternatives being $a_0 < a$. The consignment is accepted when the trial of a sample of lamps furnishes sufficient evidence for the rejection of the hypothesis tested.

The tests of both symmetrical and asymmetrical forms of "Student's" hypothesis are based on the same criterion,

$$t = (x - a)/s \tag{3}$$

and in both cases we may conveniently use the tables provided by R. A. Fisher.[2] These tables give the values of t, say t_P, such that the probability of getting by chance a value of t within the limits $-t_P < t < t_P$ is equal to $1 - P$. The test of the symmetrical hypothesis H_s consists now in a rule of accepting H_s when the observed value of t is such that $|t| < t_P$ and in rejecting H_s in other cases, i.e. when $|t| \geqq t_P$. It has been shown by Neyman and Pearson[3] that the probability of unjustly rejecting a true hypothesis when applying this rule cannot exceed the value P, which can be chosen in advance.

The test of asymmetrical hypothesis H_a is similar. We reject H_a when the observed value of t is greater than t_P and we accept H_a in other cases. The difference consists in that H_s is rejected both when $-t \geqq t_P$ and when $t \geqq t_P$ while H_a will be rejected only if $t \geqq t_P$. The consequence of this is that the probability of a false rejection of an asymmetrical hypothesis, H_a, when comparing the observed value of t with t_P will be smaller and could not exceed the limit $\frac{1}{2} P$.

Errors that consist of the unjust rejection of a hypothesis are called errors of the first kind. Those that consist of the acceptance of a

[2] R. A. Fisher: *Statistical Methods for Research Workers*, London, 1934.

[3] J. Neyman and E. S. Pearson: (a) "On the Use and Interpretation of Certain Test Criteria . . ." *Biometrika*, Vol. XX-A, 1928, pp. 175–240 and 264–294; (b) "The Testing of Statistical Hypotheses in Relation to Probabilities *a priori*," *Proc. Camb. Phil. Soc.*, Vol. XXIV, 1933, pp. 492–510.

hypothesis when it is in fact false are called errors of the second kind. Usually the tests are so arranged as to have a certain fixed limit, say α which the probabilities of first kind errors could not exceed. This limit could be such as $\alpha = 0.05$, $\alpha = 0.01$, etc. Assume that the number α, which we shall call the level of significance, is fixed. Then if we test a symmetrical "Student's" hypothesis, H_a, we should use t_P corresponding to $P = \alpha$. On the other hand, when we test the "Student's" hypothesis in its asymmetrical form, then we should use t_P corresponding in Fisher's tables to $P = 2\alpha$.

Let us now consider more closely the errors of the second kind and their probabilities. Their importance as a basis for a rational choice among possible criteria to test a hypothesis has been recognized for some time. The first table of the probabilities of errors of the second kind in testing "Student's" asymmetrical hypothesis was published by St. Kołodziejczyk.[4] A somewhat fuller table appeared last year.[5] Since then it has become evident that the rational planning of experiments to be dealt with statistically requires a table somewhat different in form from those already published. Such a table is provided in the present publication. This table, as well as the two mentioned above, has been calculated in the joint Laboratories, the Biometric Laboratory, Nencki Institute, and the Statistical Laboratory, Central College of Agriculture, Warsaw. All the tables deal with probabilities of errors of the second kind in testing "Student's" hypothesis in its asymmetrical form.

As we have mentioned, an error of the second kind occurs when we accept the hypothesis tested H_a, say that $a \leqq a_0$, while it is false and thus the true value of the population mean $a = a_1 > a_0$. It is obvious that both the importance and the probability of such errors must depend upon the value of the difference $a_1 - a_0 = \Delta$ (say). The larger the difference Δ, the more important it is to avoid the acceptance of H_a and, all other things being equal, the smaller is the probability of accepting H_a. It follows easily from the usual "normal theory" that the probability P_{II} of accepting H_a when the true value of $a = a_1$, is given by the integral

(4)
$$P_{II} = \frac{n^n}{\sigma^{n+1}\sqrt{2\pi}} \frac{1}{2^{(n-1)/2}\,\Gamma(\frac{1}{2}n)} \int_0^\infty s^{n-1} e^{-ns^2/2\sigma^2} \cdot \int_{-\infty}^{a_0+ts} e^{-(x-a_1)^2/2\sigma^2} dx ds$$

[4] St. Kołodziejczyk: "Sur l'erreur de la seconde catégorie dans le problème de 'Student'," *Comptes Rendus of the Paris Academy*, Vol. 197, 1933, p. 814.

[5] J. Neyman with co-operation of K. Iwaszkiewicz and St. Kołodziejczyk: "Statistical Problems in Agricultural Experimentation," *Supplement to the Journal of the Royal Statistical Society*, Vol. II, Part II, 1935, pp. 107–180.

which reduces to

$$P_{II}(\alpha,\rho) = \left(\frac{n}{t}\right)^n \frac{1}{2^{(n-1)/2}\Gamma(\frac{1}{2}n)} \int_0^\infty v^{n-1} e^{-nv^2/2t^2} \frac{1}{\sqrt{2\pi}} \int_{-\infty}^{v-\rho} e^{-u^2/2} du dv \tag{5}$$

where

$$\rho = (a_1 - a_0)/\sigma = \Delta/\sigma \tag{6}$$

may be called the standardized size of the error of the second kind, and $t = t_P$ is taken from Fisher's table so as to correspond to the chosen level of significance α. In Fisher's table, $P = 2\alpha$.

All the three tables mentioned were computed by calculating the integral (5) by Weddle's formula for different values of n and ρ and by backward interpolation.

The present table differs from the others in that it gives the values of ρ, the standardized size of the error of the second kind corresponding to fixed values of $P_{II}(\alpha_1\ \rho)$, α and n, while in other tables ρ was considered as an independent variable. In other words, the other tables give the answer to the question: What is the chance of accepting the hypothesis H_a that $a \leqq a_0$ when the true value of a is $a_1 = a_0 + \rho\sigma$?

On the other hand the present tables answer the question: What is the standardized size of the difference Δ between the true population mean a_1 and the hypothetical upper limit a_0 which will be undetected with the given frequency P_{II}?

The present tables are divided into two parts, Table I and Table II corresponding to the levels of significance $\alpha = 0.05$ and $\alpha = 0.01$. Rows in each table correspond to different values of the number of degrees of freedom n and the columns to several fixed values of the probability of errors of the second kind.

The use of the tables may be illustrated by the following two examples.

Example III. A breeder of new varieties of cereals considers the layout of an experiment in which he desires to compare a new variety, V_1, with an established standard V_0. Let ξ_0 and ξ_1, denote the true mean yields of the standard and the variety tested per some unit of area. The breeder will have to test the hypothesis H_a that $\Delta = \xi_1 - \xi_0 \leqq 0$ and will consider his problem of producing a better variety as successfully accomplished whenever he gets the evidence that the hypothesis is not true and therefore that $\xi_1 > \xi_0$. Assume that the level of significance is $\alpha = 0.01$, so that the breeder desires to reduce the probability of his unjust rejection of the hypothesis tested to a level not exceeding $\alpha = 0.01$. Suppose further that the experiment planned originally as-

TABLE I—P_{II} ($\alpha = 0.05$)

THE QUANTITY TABLED IS THE STANDARDIZED ERROR OF THE SECOND KIND, ρ, WHICH WILL BE COMMITTED WITH THE RELATIVE FREQUENCY P_{II}, IF THE ESTIMATE OF STANDARD ERROR IS BASED ON n DEGREES OF FREEDOM AND IF THE LEVEL OF SIGNIFICANCE ASSUMED IS $\alpha = 0.05$.

n	0.01	0.05	0.10	0.20	0.30	0.40	0.50	0.60	0.70	0.80	0.90
1	16.46	12.53	10.51	8.19	6.63	5.38	4.31	3.35	2.46	1.60	0.64
2	6.88	5.52	4.81	3.98	3.40	2.92	2.49	2.07	1.63	1.15	0.50
3	5.47	4.46	3.93	3.30	2.85	2.48	2.13	1.79	1.43	1.02	0.45
4	4.95	4.07	3.60	3.04	2.64	2.30	1.99	1.67	1.34	0.96	0.43
5	4.70	3.87	3.43	2.90	2.53	2.21	1.91	1.61	1.29	0.92	0.42
6	4.55	3.75	3.33	2.82	2.46	2.15	1.86	1.57	1.26	0.90	0.41
7	4.45	3.67	3.26	2.77	2.41	2.11	1.82	1.54	1.24	0.89	0.40
8	4.38	3.62	3.21	2.73	2.38	2.08	1.80	1.52	1.22	0.88	0.40
9	4.32	3.58	3.18	2.70	2.35	2.06	1.78	1.50	1.21	0.87	0.39
10	4.28	3.54	3.15	2.67	2.33	2.04	1.77	1.49	1.20	0.86	0.39
11	4.25	3.52	3.13	2.66	2.32	2.02	1.75	1.48	1.19	0.86	0.39
12	4.22	3.50	3.11	2.64	2.30	2.01	1.74	1.47	1.19	0.85	0.38
13	4.20	3.48	3.09	2.63	2.29	2.00	1.74	1.47	1.18	0.85	0.38
14	4.18	3.46	3.08	2.62	2.28	2.00	1.73	1.46	1.18	0.84	0.38
15	4.17	3.45	3.07	2.61	2.27	1.99	1.72	1.46	1.17	0.84	0.38
16	4.16	3.44	3.06	2.60	2.27	1.98	1.72	1.45	1.17	0.84	0.38
17	4.14	3.43	3.05	2.59	2.26	1.98	1.71	1.45	1.17	0.84	0.38
18	4.13	3.42	3.04	2.59	2.26	1.97	1.71	1.45	1.16	0.83	0.38
19	4.12	3.41	3.04	2.58	2.25	1.97	1.71	1.44	1.16	0.83	0.38
20	4.12	3.41	3.03	2.58	2.25	1.97	1.70	1.44	1.16	0.83	0.38
21	4.11	3.40	3.03	2.57	2.24	1.96	1.70	1.44	1.16	0.83	0.38
22	4.10	3.40	3.02	2.57	2.24	1.96	1.70	1.44	1.16	0.83	0.37
23	4.10	3.39	3.02	2.56	2.24	1.96	1.70	1.43	1.15	0.83	0.37
24	4.09	3.39	3.01	2.56	2.23	1.95	1.69	1.43	1.15	0.83	0.37
25	4.09	3.38	3.01	2.56	2.23	1.95	1.69	1.43	1.15	0.83	0.37
26	4.08	3.38	3.01	2.55	2.23	1.95	1.69	1.43	1.15	0.82	0.37
27	4.08	3.38	3.00	2.55	2.23	1.95	1.69	1.43	1.15	0.82	0.37
28	4.07	3.37	3.00	2.55	2.22	1.95	1.69	1.43	1.15	0.82	0.37
29	4.07	3.37	3.00	2.55	2.22	1.94	1.68	1.42	1.15	0.82	0.37
30	4.06	3.37	2.99	2.54	2.22	1.94	1.68	1.42	1.15	0.82	0.37
$+\infty$	3.97	3.29	2.93	2.49	2.17	1.90	1.64	1.39	1.12	0.80	0.36

TABLE II—P_{II} ($\alpha = 0.01$)

THE QUANTITY TABLED IS THE STANDARDIZED ERROR OF THE SECOND KIND, ρ, WHICH WILL BE COMMITTED WITH THE RELATIVE FREQUENCY P_{II}, IF THE ESTIMATE OF STANDARD ERROR IS BASED ON n DEGREES OF FREEDOM AND IF THE LEVEL OF SIGNIFICANCE ASSUMED IS $\alpha = 0.01$

n	0.01	0.05	0.10	0.20	0.30	0.40	0.50	0.60	0.70	0.80	0.90
1	82.00	62.40	52.37	40.80	33.00	26.79	21.47	16.70	12.27	8.07	4.00
2	15.22	12.26	10.74	8.96	7.73	6.73	5.83	4.98	4.12	3.20	2.08
3	9.34	7.75	6.87	5.87	5.17	4.59	4.07	3.56	3.03	2.44	1.66
4	7.52	6.28	5.64	4.88	4.34	3.88	3.47	3.06	2.63	2.14	1.48
5	6.68	5.62	5.07	4.41	3.93	3.54	3.17	2.81	2.42	1.98	1.38
6	6.21	5.25	4.74	4.13	3.70	3.33	2.99	2.66	2.30	1.88	1.32
7	5.91	5.01	4.53	3.96	3.55	3.20	2.88	2.56	2.22	1.82	1.27
8	5.71	4.84	4.39	3.84	3.44	3.11	2.80	2.49	2.16	1.77	1.24
9	5.56	4.72	4.28	3.75	3.36	3.04	2.74	2.43	2.11	1.74	1.22
10	5.45	4.63	4.20	3.68	3.31	2.99	2.69	2.39	2.08	1.71	1.20
11	5.36	4.56	4.14	3.63	3.26	2.94	2.65	2.36	2.05	1.69	1.18
12	5.29	4.50	4.09	3.58	3.22	2.91	2.62	2.33	2.03	1.67	1.17
13	5.23	4.46	4.04	3.55	3.19	2.88	2.60	2.31	2.01	1.65	1.16
14	5.18	4.42	4.01	3.51	3.16	2.86	2.57	2.29	1.99	1.64	1.15
15	5.14	4.38	3.98	3.49	3.14	2.84	2.56	2.28	1.98	1.63	1.14
16	5.11	4.35	3.95	3.47	3.12	2.82	2.54	2.26	1.97	1.62	1.14
17	5.08	4.33	3.93	3.45	3.10	2.80	2.53	2.25	1.96	1.61	1.13
18	5.05	4.31	3.91	3.43	3.08	2.79	2.51	2.24	1.95	1.60	1.13
19	5.03	4.29	3.89	3.41	3.07	2.78	2.50	2.23	1.94	1.60	1.12
20	5.01	4.27	3.88	3.40	3.06	2.77	2.50	2.22	1.93	1.59	1.12
21	4.99	4.25	3.86	3.39	3.05	2.76	2.49	2.22	1.93	1.59	1.12
22	4.97	4.24	3.85	3.38	3.04	2.75	2.48	2.21	1.92	1.58	1.11
23	4.96	4.23	3.84	3.37	3.03	2.74	2.47	2.20	1.91	1.58	1.11
24	4.94	4.21	3.83	3.36	3.02	2.73	2.47	2.20	1.91	1.57	1.11
25	4.93	4.20	3.82	3.35	3.02	2.73	2.46	2.19	1.90	1.57	1.10
26	4.92	4.20	3.81	3.34	3.01	2.72	2.45	2.19	1.90	1.57	1.10
27	4.91	4.19	3.80	3.34	3.00	2.72	2.45	2.18	1.90	1.56	1.10
28	4.90	4.18	3.79	3.33	3.00	2.71	2.44	2.18	1.89	1.56	1.10
29	4.89	4.17	3.79	3.32	2.99	2.71	2.44	2.17	1.89	1.56	1.09
30	4.88	4.16	3.78	3.32	2.99	2.70	2.44	2.17	1.89	1.55	1.09
$+\infty$	4.65	3.97	3.61	3.17	2.85	2.58	2.33	2.07	1.80	1.48	1.04

sumes that each of the varieties V_0 and V_1 (among some others) will be replicated say $n'=8$ times, and that the number of degrees of freedom on which the estimate of the standard error will be based will be $n=14$. According to previous experience the standard error of any single yield is expected to be say $\sigma_0=6$ per cent of the general mean yield. Having all these data the experimenter will be naturally interested in the size of differences between the mean yields of varieties V_0 and V_1 (in favour of the new variety V_1) which he is likely to detect in his experiment in case they in fact exist. It is in fact obvious that the new variety may be better than the standard, V_0, but owing to the random variation its advantage may remain undetected. The answer to the question asked is contained in Table II in the row corresponding to $n=14$, which gives the standardized second kind error ρ corresponding to several values of the probability P_{II}. The difference Δ will be estimated by the difference, say $\delta=\bar{x}_1-\bar{x}_0$ of the mean yields of two varieties V_0 and V_1 from eight plots each. The standard error of δ will be $\sigma=\sigma_0\sqrt{2/n'}$ or, in this case, $\sigma=3$ per cent of the general mean yield. Now $\Delta=\rho\sigma=3\rho$ per cent of the general mean. Substituting the figures in the row $n=14$ of Table II instead of ρ in the above formula we shall get the following result describing the efficacy of the experiment planned:

TABLE A. LEVEL OF SIGNIFICANCE $\alpha=0.01$

Size of real difference in average yields in percentage of the average yield	15.54	13.26	12.03	10.53	9.48	6.87	5.97	4.92	3.45
Chance of non-detection	0.01	0.05	0.10	0.20	0.30	0.60	0.70	0.80	0.90

In other words, if the true excess in the average yield of variety V_1 over that of the standard variety V_0 is as large as 15.54 per cent of the general mean yield, then the experiment described will fail to detect this difference only in 1 per cent of cases. If the true difference in yields is about 13.26 per cent of the general mean, then the chance of its being not detected by the experiment is 0.05, etc. It will be seen that a reasonable chance of detection such as 0.9 or 0.8 corresponds to true differences in yields exceeding 10 per cent of the general mean yield and that the differences under 5 per cent will be left undetected very frequently, more than in 80 per cent of all cases.

Now the experimenter is able to judge whether the efficacy of the proposed trial is sufficient. If a gain in yield amounting to 5 per cent is considered negligible and if it is really essential to discover only such differences as 10 per cent or over, then he will judge that the experiment planned is satisfactory. If, however, the process of improving the varieties he is concerned with is already fairly advanced, so that

it is more or less hopeless to look for a gain in yield as large as 10 per cent, then probably he will make some efforts to assure a greater chance of detecting smaller differences, when they exist. This may be achieved in three ways, separately or in combinations.

(1) We may increase the number of replications of each variety and consequently also the number of degrees of freedom.

(2) We may use a less stringent level of significance, say $\alpha = 0.05$ instead of $\alpha = 0.01$ proposed previously. This will lead to an increase in the chance of error when stating that V_1 does give better yields than the standard variety V_0, but it will also increase the chance of detecting the advantage of V_1 when it exists. To illustrate this point we give the following Table B similar to Table A and describing the efficacy of the same experiment under the assumption that $\alpha = 0.05$.

TABLE B. LEVEL OF SIGNIFICANCE $\alpha = 0.05$

Size of real differences in average yields in percentage of the average yield	12.54	10.38	9.24	7.86	6.84	4.38	3.54	2.52	1.14
Chance of non-detection	0.01	0.05	0.10	0.20	0.30	0.60	0.70	0.80	0.90

It is seen that applying a more liberal level of significance we shall have a reasonable chance of detecting a difference in yield such as 8 per cent and that the differences which will be overlooked in 80 cases out of 100 and over are those below 2.52 per cent of the general mean. Thus the detecting power of the experiment is considerably increased.

(3) The third and the most desirable way of increasing the efficacy of the experiment consists in lowering the standard error σ of any single plot yield, i.e. in improving the experimental technique.[6]

Example IV. Let us consider more closely the situation outlined in Example II. Suppose then that the buyer of some manufactured product insists on having evidence that the mean value a of some character X of individual units in a consignment exceeds a fixed limit a_0. We have seen already that such "evidence" could result from the inspection of a sample drawn from the consignment and from the test of an asymmetrical hypothesis, H_a, that $a \leqq a_0$. If after having applied the test this hypothesis is rejected, then the buyer will accept the consignment. Errors in testing consignments are, of course, inevitable, but suppose that the buyer does not want to be wrong in accepting unsatisfactory consignments more often than with the relative frequency, say one in a hundred. This is easily achieved by fixing the level of significance $\alpha = 0.01$. The choice of the proper level of significance entirely protects the interests of the buyer, and other details

[6] Some indications of how this may be achieved will be found in the joint paper of Neyman, Iwaszkiewicz and Kolodziejczyk already referred to.

of the test of the consignments of goods are more or less indifferent for him. But now consider the problem from the point of view of the producer. It is obvious that the manufactured products may have in fact the average value of the character X well above the fixed limit a_0 and then, through chance causes, the consignment may not pass the test, thus causing considerable financial loss. It is therefore in the interests of the producer to assure that the products he is delivering should have a reasonable chance of passing successfully the test.

Since the publication of the well known book of Shewhart[7] there will be scarcely any larger works which would not apply the modern methods of inspection. We may therefore assume that the manufacturer knows approximately the value of the standard deviation, σ_0, of the character X of single units in consignments he is delivering and also that he is able to direct the process of manufacturing so as to obtain consignments with the average value of this character having approximately any desired (and attainable) value a. We shall now show how he may use the Tables I and II in order to assure a reasonable chance for his goods to pass the test of the buyer.

Denote by n' the number of single units in the sample, which will be inspected, and by $\bar{x}$ the mean of the character X which will be calculated for the sample. The standard error of $\bar{x}$ will be equal to $\sigma_{\bar{x}}=\sigma_0/\sqrt{n'}$ and may be assumed to be approximately known. The number of degrees of freedom on which the estimate of $\sigma_{\bar{x}}$ will be based is $n=n'-1$. Now the values of ρ for $\alpha=0.05$ and 0.01 come from Tables I and II; and the relation between ρ and the values of a_0, a, σ_0 and n' is

$$\rho = (a - a_0)/\sigma_{\bar{x}} = (a - a_0)\sqrt{n'}/\sigma_0 \tag{7}$$

from which

$$a = a_0 + \rho\sigma_0/\sqrt{n'}. \tag{8}$$

Substituting here for ρ the figures from the row corresponding to the $n=n'-1$ of Table II, we shall find what should be the true value of the average a so as to reduce the chance of an unjust rejection of the consignment to some specified level, say 0.01 or 0.05, etc.

Assume, for example, that $a_0=100$, $\sigma_0=10$ and that the proposed value of n' is $n'=16$. Then $n=n'-1=15$ and if the producer desires that the chance of his goods passing successfully the test should be at least 0.95, he should aim at the average value of the character X equal to

$$a = 100 + 4.38(10/4) = 110.95. \tag{9}$$

If the true average value of X in his consignments is still exceeding

[7] W. A. Shewhart: *The Economic Control of Quality of Manufactured Product.* New York, 1931.

$a_0 = 100$ but is lower than the above limit, 110.95, the consignments will be rejected more frequently. For example, the frequency of unjust rejection equal to 0.5 will correspond to consignments in which the true average of X is equal to

$$a = 100 + 2.56(10/4) = 106.4. \tag{10}$$

According to circumstances, the effort of producing goods of an average quality exceeding the desired level by 11 per cent may be too costly and the producer may prefer to bear the costs of a more stringent test instead. It will be seen that if the sample to be tested is increased to contain say 64 individual units instead of 16, then the number of degrees of freedom will be 63 and in order to have the chance of 95 per cent of the consignment passing the test, the producer will have to aim at the average value of X in the consignments contained between the following limits say

$$a_1 = 100 + 4.16(10/8) > a > 100 + 3.97(10/8) = a_2 \tag{11}$$

or

$$105.20 > a > 104.97, \tag{12}$$

where the numbers a_1 and a_2 correspond to the numbers of degrees of freedom $n = 30$ and ∞ respectively.

The examples considered above illustrate the simplest cases where the Tables I and II could give useful information. However, it will be recognized that the range of application of these tables is extremely broad. They will be particularly useful when discussing more or less complex experiments leading finally to statistical test of "Student's" hypothesis of asymmetrical type.[8] In many a case it will be found that a particular experiment would be useless because of the exceedingly small chance of detecting effects which are likely to be met with and which are of practical value.

Before concluding we must call attention to the fact that the tables assume normality in the populations sampled. However, excellent sampling experiments by E. S. Pearson showed that if the actual distributions show but moderate departures from normality, the effect of this is in many cases negligible.[9] Another point which seems to be worth while mentioning concerns the possibility of using the tables when the hypothesis tested is symmetrical. It will be easily seen that in that case the tables will be *approximately* valid too, except that instead of the levels of significance $\alpha = 0.05$ and $\alpha = 0.01$ they will correspond to $\alpha = 0.10$ and $\alpha = 0.02$ respectively. The approximation will be better for smaller values of P_{II} than for the larger ones.

[8] See, for example, *Supplement to the J.R.S.S.*, Vol. II, 1935, pp. 235–241.

[9] E. S. Pearson: "Statistical Methods in the Control and Standardization of the Quality of Manufactured Products." *J.R.S.S.*, Vol. XCVI, Part I, 1933.

THE STATISTICAL VIEW OF NATURE*

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It is very gratifying for one who is neither a statistician nor a physicist to be asked to speak to you on statistical concepts and the physical world. I take it that you have chosen this topic because you feel that recent changes in the fundamental ideas of physics and especially Heisenberg's Indeterminacy Principle have some bearing on your work, and that you have invited a philosopher rather than a physicist or a statistician because you wish to consider not technical details but how the whole enterprise appears to one who stands outside of both physics and statistics and tries to observe the main currents in both. I shall therefore limit myself to a consideration of elementary ideas, in the belief that the rudiments need, and can stand, eternal repetition.

Since the beginning of modern statistical science, its leaders such as Quetelet have in agreement with the pioneers of the other social sciences insisted that progress is possible only if we introduce the rigorous and deterministic methods which have brought such great success to the physical sciences. And now we are witnessing physical science itself not only raising doubt as to the adequacy of Newtonian mechanics and the law of the conservation of energy, but also challenging the entire classical conception of physical causation. This has caused considerable disturbance in the social sciences; but certain philosophers, both positivists and those of theologic leaning, are rejoicing. For the latter have been asserting for some time that our knowledge of physics is statistical only, of the same kind as our knowledge of social phenomena when these are the result of large numbers.

To discuss the elements of this issue, I shall begin by considering the characteristics of statistical knowledge, and then try to analyze the nature of statistical mechanics and the significance of determinism in a statistical view of the world. I shall conclude with venturing some application as to the limitations of statistical measurement. Before doing so, it is interesting to note how many fundamental terms which the social sciences are trying to adopt from physics have as a matter of historical fact originated in the social field. Take, for instance, the notion of *cause*. The Greek *aitia* or the Latin *causa* was originally a

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purely legal term. It was taken over into physics, developed there, and in the 18th century brought back as a foreign-born king for the adoration of the social sciences. The same is true of the concept of *law of nature*. Originally a strictly anthropomorphic conception, it was gradually depersonalized or dehumanized in the natural sciences and then taken over by the social sciences in an effort to eliminate final causes or purposes from the study of human affairs. It is therefore not anomalous to find similar transformations in the history of such fundamental concepts of statistics as *average* and *probability*. The concept of *average* was developed in the Rhodian laws as to the distribution of losses in maritime risks. After astronomers began to use it in correcting their observations, it spread to other physical sciences; and the prestige which it thus acquired has given it vogue in the social field. The term *probability*, as its etymology indicates, originates in practical and legal considerations of probing and proving. And now we are trying to fit social phenomena into the patterns of probability worked out by mathematical physicists such as LaPlace and Poisson, and by mathematicians like Gauss and Karl Pearson.

I

What are the essential traits that differentiate statistical from other kinds of knowledge? It is usual to discriminate it from history on one hand and from mechanics on the other.[1] These distinctions are worth noting.

Historical knowledge is concerned with individual objects or events, with individual persons, nations, or institutions having a definite date or position in time. Statistical knowledge differs in this respect by being concerned with numerical relations in multitudes which are fungible, i.e. in which individual differences are ignored. To know that the population of the United States in 1800 was 4,000,000 is, to be sure, historical in so far as it describes the United States at that time and indicates its power relative to other nations, but it is statistical in respect to the individuals who constitute that population. It gives us no information about any one of them. And this ignorance is not cured by averaging. If I am told that the average family then consisted of 5.6 persons, I do not thereby learn anything about any actual family. We are not thus denying that statistical information is a necessary aid in the understanding of history. To determine what was the population of Athens in 430 B.C. or of the United States in 1790 is surely a most significant task of history; but we do not thereby attain direct

[1] The reader will note that the text distinguishes between different kinds of *knowledge*. The scientific statistician, of course, must use all the kinds of knowledge he can obtain, including pure mathematics which is certainly not in itself statistical.

information about any individual citizen, what were the peculiar traits that made some of them, like Pericles or Washington, such outstanding figures. This distinction between the historical and the statistical is also valid in the realm of physics. If I know that there was an earthquake or an eclipse of the moon or that a column of mercury attained a certain height on a given occasion, my knowledge is so far purely historical. And from a logical point of view our knowledge of the geologic changes which our earth has undergone is of the same logical pattern as our knowledge of the changes in the dynasties which have ruled Egypt. In both cases we reconstruct individual past events on the basis of observations of presently existing records or remains. On the other hand, our knowledge of physical nature is strictly statistical whenever it is concerned with averages such as heights or weights of groups of objects or with the relative frequencies with which certain phenomena occur.

The distinction between statistical and mechanical knowledge deserves even greater attention. The classical view held generally throughout the 18th and 19th centuries regarded the principles of Newtonian mechanics, like those of Euclidean geometry, as self-evident or in some way necessary. Thus, philosophers like Kant, Whewell, and Wundt have tried to derive the Newtonian laws of mechanics from entirely a priori considerations. Also, great physicists like J. J. Thomson, the successor of Maxwell, explicitly declared the laws of dynamics to be in some sense superior to those which like the second law of thermodynamics, are merely based on experience. But the general distrust of self-evident principles that has come with non-Euclidean geometry and the recent development of non-Newtonian mechanics makes this position a difficult one to maintain. All the ultimate laws of nature which we know are contingent, that is, we know of no reason why these laws should prevail in fact rather than others which are conceivable; and the really decisive evidence for the truth of the laws of any mechanical system is that their consequences are verifiable, that is, agree with experimental observations. Yet though mechanical laws of the classical type are not justified by their self-evidence and are not independent of experience, they can be clearly distinguished from statistical laws by the fact that they *assert universal or invariant connections in nature.* Whenever we have two particles, they attract each other with a force that varies inversely as the square of the distance between them; or, whenever a body moves freely under the influence of a constant force, the velocity acquired will be proportional to the time during which the force acts. A single exception can thus logically refute the law. Now, statistical laws or correlations do not take such

universal form, but assert rather certain frequencies, for example, that a little over 51 per cent of all births are male. This, however, does not enable us to predict individual instances. To say that 75 per cent of the Romans were tall, e.g., above 5′ 5″, gives us no warrant for inferring that Cato was tall. And to say that this information makes it probable that Cato was tall does not really determine the individual case, for he may well have been only 5′ 4″ despite the fact that so many of his fellow-countrymen were taller. This makes a tremendous difference in the problem of verification. A universal proposition asserts something to be impossible and if that which is thus excluded does take place our proposition is definitely refuted. It is thus always conceivable and sometimes actually the case that a single crucial experiment can refute an hypothesis or supposed law. And if another hypothesis is thus confirmed, the latter may be said to be verified in the sense that it has been shown to be in greater harmony with experimental observation than its rival or competing hypothesis. Thus, if the classical theory predicts an observable motion relative to the aether, and such motion does not show itself under proper conditions of observation in the Michelson-Morley experiment, we have definite evidence against the classical theory. And if the Einstein theory of relativity can explain this experiment as well as all that the classical theory does, we have definite reasons for preferring it. Statistical generalizations, however, cannot be so readily verified by any crucial experiment. Consider for instance, the well established statistical observation that as the number of voters increases, the pluralities in favor of some candidate increase also. According to this view, there is a negligible probability that the millions of voters of New York State will in the next election be exactly evenly divided between the Democratic and Republican candidates for Governor. If, however, that should actually happen, it will not refute our statistical generalization since the latter does not declare a tie vote under these conditions to be impossible.

It may well be objected that this sharp distinction between mechanical and statistical laws in regard to verification, overlooks the fact that statistical laws apply not to individual instances but only to large groups or over a long run, and that when so taken they are verifiable. Thus, consider the classical statistical law referred to before as to the ratio of male to female births. If, for a long period and over a wide area this ratio is not found to prevail, we should surely regard it as refuted by experience, and some other ratio might be verified. This objection, however, while perfectly true does not fully obviate the difficulty to which we have pointed. For the test as to when a run is sufficiently long to verify a statistical law cannot in the nature of the

case be very precise. A few repetitions of the Michelson-Morley experiment are necessary to make certain that we have not overlooked something. But they are repetitions of what is essentially the same experiment which if true is sufficient to overthrow the contradictory universal. But no one can well determine just how many reports of different cities for different years would be necessary to refute the statistical law referred to above.

I do not wish to ignore difficulties in pressing the foregoing point. The history of science undoubtedly shows that crucial experiments are very infrequent, and that they do not play the absolutely decisive role which popular histories of science ascribe to them. No single experiment overthrew the Ptolemaic astronomy. Galileo's supposed refutation of Aristotle's theory of gravitation by throwing two objects from the tower of Pisa is rather mythical, for it does not seem that under the actual conditions of height and atmospheric friction and the means of observation, this experiment could have been decisive. Despite Lavoisier's experiments, the phlogiston theory continued to be held for a long time. Nevertheless, the issues in each of these cases were definitely formulated because universal laws were involved and single facts, such as the observed phases of the planet Venus or the increased weight of certain substances after being burned, were definitely and directly contrary to the predictions. If a single fact is not generally sufficient to overthrow completely a supposed universal law which it contradicts, it is because an established hypothesis generally rests not on a single chain of arguments but on many interwoven threads, so that the cutting of one of these threads is not at once fatal. Moreover, theories are flexible and can be reformulated, or have auxiliary hypotheses added, e.g., Lavoisier's experiments can be explained by endowing phlogiston with negative weight. But in any case every single theoretic statement in regard to nature takes the form of universal propositions which can be definitely refuted by a single contradictory instance and this is not true of statistical statements.

Another way of stating the distinction between mechanical and statistical laws is that the former state causal relations while the latter are mere correlations. It is not necessary to go into the traditional discussions as to the nature of causality. It is sufficient for our present purpose to insist that a causal relation means some intimate connection or thread of identity between two things, traits or events thus connected, while a statistical correlation may mean nothing more than a temporary coincidence. The fallacious assumption that a high correlation must necessarily represent a causal relation, while not held by scientific statisticians, is still widely current. In a book of mine published

a few years ago, I referred to Dr. George Marshall who found a correlation of 87 per cent between the membership of the International (really American) Machinists' Union and the death rate of the state of Hyderabad. Many of my readers have since protested vigorously that such a high correlation extending over twelve years cannot be accidental or devoid of real significance. But the fact is that the correlation does not hold beyond the period taken. Indeed, it obviously could not have prevailed before the Machinists' Union was formed when there was still a considerable death rate in that Hindu State. If more instances of this sort are not available it is because most of us have no interest in such purely logical demonstrations. We look for correlations where we suspect a real connection and then we regard whatever small correlation we find as a proof of our hypothesis. Where there are genuine causal connections we should expect statistical correlation, but the converse is not necessarily true. Genuine physical and mechanical laws are thus more than statistical correlations. The law of the conservation of energy and Newton's law of motion, are all empirical in the sense that they depend for their verification on observation, but they also indicate some element of identity in cause and effect, or antecedent and consequent, and that is more than mere statistical correlation. The observation that a certain correlation prevails for a certain period does not of itself give us reason for supposing that it prevailed before we observed it or that it will do so in the future. But scientific procedure in physics cannot go on without assuming that the relations we deal with prevailed before we discovered them and are independent of our knowing them. Careful statisticians recognize this when they draw a distinction between gross and refined statistics, between birth rates where the denominator is the total population and those based on the number of married women of child-bearing age. Such distinctions are based on the recognition that causal relations are necessary to give statistical correlations real significance.

The foregoing reflections suggest one of the reasons why there is in fact much less consensus in regard to social than physical laws as statements of invariant or causal relations. If we formulate a theory of business cycles or of the variations of the stock market, and the actual facts do not conform, we do not generally abandon the theory, but we explain the departure of the facts by means of disturbing factors that prevent a fair test of our assumed law. Now, the existence of perturbations is also found in physics. But in the latter the effect of disturbing forces can generally be measured independently. That, however, is generally impossible when as in the case of the weather or of biologic and social phenomena generally, we have a large number

of factors that are not independent of each other. Hence, in dealing with such complex phenomena we can seldom prove that if a given disturbing factor were absent, our assumed law would operate. To attain invariant relations we must be able to isolate our factors or vary one at a time. Otherwise, we have too many variables and no assumption of ours can be definitely verified. The analysis of any situation into its related factors is therefore the condition for any definite test as to the truth of even statistical generalizations.

It is important to remember that we do not always get nearer to the truth by increasing the number of cases on which our generalization is based. Observations on fifty Chinese laundrymen no more justify the generalization as to Chinese in general than an observation of twenty Chinese, if they all come from Canton, all belong to the same society, etc. It is only when by good fortune the randomness of our samples does eliminate the fallacy of selection that a larger number gives better evidence than a smaller number. In the end, the truth of a generalization from a sample depends on the homogeneity of the group with respect to which we wish to generalize. A single experiment on a new substance, to test whether it is acid or alkaline, is much more convincing than the result of a questionnaire addressed to millions of army men to measure their intelligence. For the latter is not a simple quality of a uniformly repeatable pattern. In this respect, the methods of social statistics are gross compared with the refinements of analysis, so that when our analysis is thoroughgoing, as it generally is in physics, one or two samples are as good as million. If what we are measuring is really homogeneous, one instance is sufficient. In the social field, therefore, too, statistics cannot take the place of analysis; and in fact acute social analysts have contributed much more to our understanding of social phenomena than those who, without genius or vision, have believed that the mere collection of instances will give us adequate knowledge.

Statistical method has been closely associated with the belief in what is loosely called induction, and it is often asserted that the founders of statistical science were men who, like Sir William Petty and those who organized the Royal Society, were influenced by Francis Bacon. It would take me far afield to show the mythical character of this history. A few observations, however, are essential. There can be no doubt that statistics deals with actuality, and that knowledge of actualities is always empirical, i.e., that we cannot obtain knowledge of existence by purely a priori methods. There is, however, no genuine progress in scientific insight through the Baconian method of accumulating empirical facts without hypotheses or anticipation of

nature. Without some guiding idea we do not know what facts to gather. Without something to prove, we cannot determine what is relevant and what is irrelevant. If we wish to find the laws of the weather, the thousands of millions of observations will not of themselves indicate the true causes of laws. Only when we strike some fruitful idea or hypothesis such as the late Carl Barus did in determining the influence of dust on the concentration of moisture, can we make some progress in the understanding of nature. Economic statistics, charts of the variations of prices, incomes, imports and exports, etc., do not constitute science unless organized, controlled and informed by some general idea. A collection of data will not give us science any more than a collection of ores will give us metal works. We need fire to fuse our material into some pattern. In science we achieve that, if we discover the proper perspective from which the order of phenomena becomes visible to the trained eye. This does not mean that we do not often go wrong because of false hypotheses, but it is well to recall the caution of a great naturalist, viz., Charles Darwin, who rightly maintained that the danger of false hypotheses is never as great as that of a false observation. For even a false hypothesis may enable us to advance the organization of our material; but everything that is based on a false observation has to be undone before we can build anew on more reliable data. But note that even in statistical work we do not dispense with a priori assumptions, that is, with assumptions which are based on general considerations and which cannot be readily refuted by empirical considerations. We can see this in our various assumptions as to what are independent or equiprobable events. I do not wish to touch on the subject of probability which will be more adequately treated by Dr. Nagel. But I must call attention to the fact that in all statistical measurement and inference we do involve ourselves in assumption as to what events are equally probable and what events are independent. And in this field priori considerations are most potent. Ask people at large, what is the probability of a penny falling head? Almost everybody answers one-half, although few have ever taken an actual count of the relative frequency with which pennies fall head or tail. The answer comes in fact not on the basis of actual observation, but on the indetermination which prevents us from giving heads a preference over tails. If, however, we ask whether Americans are as likely to pay their bills as Germans, we do not ask for data, nor are we likely to say that one is as likely as the other. The answer will largely depend on where we live. Our belief, then, in the probability of an event is largely determined by considerations other than statistical study. And this, curiously enough, has misled many trained

scientists. Thus the astronomer Proctor insists that if a penny has been thrown up fifty times and has fallen head forty times, the probability of the next throw being head is still one-half. Those who say this, however, are confusing mathematical reasoning (to wit: what follows if we make a certain assumption) with an existential assertion. If the probability of a penny falling head as often as tail were built on experience alone, any long run of tails or of heads ought to throw doubt on our assumption of independence and equiprobability. It is absurd to suppose what is established on the basis of past observation cannot be upset or modified by increased observation.

We must, therefore, reject the tradition of which Quetelet and Buckle were the high priests, and which spoke of statistical averages as if they were iron and unchangeable laws of nature beyond human control. (People still speak of the average or the mean about which our numbers oscillate as the normal man with some implication perhaps that the normal is the perfect.) Obviously there is no law of nature which compels the normal family to have 2.7 children or the normal man to divorce his wife or commit bankruptcy once in so many years. There is, therefore, also no merit in the argument that the existence of such averages proves that human volition cannot change what these averages describe. Statistical averages do give us knowledge of groups if there are in fact laws which keep the distribution in those groups approximately stable. But that empirical generalizations do not always give us genuine laws can be seen in popular generalizations about diet, exercise, the evil eye, lucky and unlucky numbers, the length of cycles on the stock market, etc. Are such generalizations more reliable if expressed in numbers, charts or curves?

As a matter of fact we know of no statistical laws comparable in constancy with the law of multiple proportion in chemistry or of planetary motion in astronomy. All the social statistical averages that we know vary somewhat, as Lexis and others have shown, from year to year and are conditioned by our social and political regulations. Many of them, e.g., the number of bank failures per annum, car loadings or automobile accidents, have no meaning except under special conditions which come and pass, while mechanical laws are independent of geography and chronology. For the very meaning of causality is that mere position in time or place cannot in itself determine a natural change.

Suppose that in the social field you find a formula like Pareto's that fits the tables of incomes for several countries for a number of years. Does it follow that it is a law which will persist and which legislation cannot change? Some have actually argued to that effect, but in point

of fact it is not true universally, and does not hold for small incomes.[2]

Statistical information needs refined analysis before it can lead to causal or rigorously scientific information. If we know that the average life of men is less than that of women we ask, is it due to occupation, habits, or to organic handicaps? We need to pass from the macroscopic to the microscopic point of view.

It is necessary to insist on these obvious reflections because of the widespread manner of speaking of the law of large numbers and the normal frequency curve as if they were a priori laws of nature governing the distribution of all possible phenomena. This popular view is logically groundless and vitiates much statistical work such as that involved in much of our intelligence testing.

Let us remember that the curve of distribution derived from the Gaussian assumptions can hold true only where these assumptions happen to be true in fact. Now, the curve assumes that our phenomena or instances are symmetrically distributed, but this is obviously not true of all phenomena even if their number is indefinitely increased. Many anthropologic measurements certainly do not conform to it, e.g., the death rate, the acceleration and retardation of growth in children, and the like. In many cases to be sure, larger groups show greater stability and more balance or symmetry than the smaller groups which constitute them, but the reverse is also true at times. The normal probability curve is a help in analysis, but as a description of nature it is true only of those groups of figures which happen to conform to it. There is no fair presumption that all groups will conform.

II

So far I have been considering the distinction between statistical laws or correlations on one hand and mechanical laws on the other. But we have with us the science of statistical mechanics. What light does it throw on our issue? Let us consider it in three stages: first, the classical form of statistical mechanics; second, the modification introduced by the quantum theory; and third, the more recent wave mechanics and the Indeterminacy Principle.

The conception of a statistical knowledge of nature was first clearly enunciated by Maxwell (though partly anticipated by Calusius) in connection with the law of entropy. In his day it was assumed that all genuine physical laws must be deducible from Newtonian mechanics. But the irreversibility of certain physical changes, e.g., the diffusion of gases or the fact that heat is conducted only from hotter to colder

[2] The difference between an empirical and a rational formula can be seen when we compare the carpenters' rule for constructing a right angle with the Pythagorean theorem. The carpenters' rule holds only for lengths of 3, 4, and 5, while the Pythagorean theorem gives us a whole system of geometric propositions or an infinite series of such possible numbers.

bodies, could not be deduced from mechanical principles concerning phenomena that are essentially reversible. This led Maxwell and Boltzmann to the idea that this irreversibility is not a fundamental law of nature holding like the laws of Newtonian mechanics always and everywhere from the largest mass to the smallest particle, but that it is a derivative result from the arrangement of molecular motions according to the curve of "error" or probability. From this point of view it is by no means impossible, though highly improbable, that some phenomena may actually take place contrary to the law of entropy. There have indeed been those who have urged that organic phenomena and even Brownian movements illustrate this possibility. It should be noted, however, that this classical system of so-called statistical mechanics does not proceed from empirical observations, by averaging and establishing correlations. Rather does it begin by assuming that there are definite laws which determine the distribution of molecular velocities in accordance with the Gaussian function originally derived to describe the distribution of errors of observation. This law of distribution is thus assumed to be an invariant law of nature, though it does not enable us to determine the position of any particular molecule. It is well to note also that statistical mechanics is still mechanics. It continues to assume the Hamiltonian principle, and still proceeds from a theoretically assumed system of mechanical laws in the microscopic realm to explain observable macroscopic phenomena. No wonder, therefore, that Maxwell could talk of statistical mechanics and yet believe in the absolute uniformity of all molecules, fresh as they issued from the hand of the Creator.

At the end of the 19th century, its leading physicist, the venerable Lord Kelvin, spoke of two clouds in the mechanical view of the universe, its difficulties in explaining the Michelson-Morley experiment and the partition of energy in the spectrum. Both of these difficulties proved insurmountable and led to revolutionary changes. The first difficulty led to the non-Newtonian mechanics of Einstein's relativity theory. The second difficulty led to the perhaps more radical departure from classical mechanics, the quantum theory of Planck and Einstein. The classical theory regarded the Universe as made up ultimately of separate particles, and yet the radiation of energy was regarded as continuous—a view which Einstein has well characterized as maintaining that the world is both soup and a bag of hard peas. Now, the actual distribution of energy in the spectrum cannot be fitted into the theory that energy radiation is continuous, and this seems to eliminate the continuity of the world which is the essence of the classical geometric view which goes back through Newton, Galileo and Kepler to the early

Greek geometers. The world consists not only of individual particles but also of multitudes of quanta of energy. This strengthens the statistical view so far as the latter proceeds by summation. The quantum theory, however, not only retains mechanical or invariant laws, such as are embodied in Hamilton's Principle, but adds new ones, to wit, exclusion principles such as Pauli's, which assert that certain states are impossible in nature.

After a series of great achievements, the quantum theory began to encounter insurmountable difficulties and a seemingly more revolutionary form of mechanics—popularly known as wave mechanics—began to take possession of the field.

From the popular viewpoint, the most impressive novelty of this mechanics is Heisenberg's Indeterminacy Principle, according to which all physical measurements are subject to an indetermination within one quantum. We have always known empirically that our measurements vary and that laws or universal propositions about the physical universe can never be absolutely verified in actual measurements. Heisenberg's Principle, however, goes farther and gives us a theoretic basis in the very nature of our mechanical laws for assigning a definite limit beyond which greater accuracy is unattainable. This in itself does not necessarily mean a denial of the principle of causality or an assertion of indeterminism in the objective physical world. It may be explained as a consequence of the fact that any measurement which involves observation of nature through light is itself a physical operation which disturbs the object observed. But be that as it may, the radical feature of the new mechanics is that its basic law or equation does not assert an invariant rule that determines the position and velocity of every individual particle, but asserts rather a probability function so that with regard to any electron or photon it can tell us only the relative frequency with which it will be found in a given position. Though called the wave mechanics, the new physics really abandons the effort at a precise picture or mechanical model of what goes on in the microscopic realm that forms the physical sub-stratum of the visible world. It thus seems to abandon at the outset the possibility of an absolutely accurate and exhaustive description of the world. The indeterminacy which it introduces into physics is the indeterminacy of passing from an average or a probability function to an individual instance. For a law which describes how a group of individuals are distributed does not give us precise knowledge of any individual constituent of the group. In this sense, the new mechanics is more conscious of the old truth that our most accurate knowledge of the physical world is never free from an element of probability. While this involves

profound technical transformations in physical science it is not very revolutionary from the philosophic point of view. The new mechanics still operates with mathematical or invariant laws which relate one state of the physical system to past or future states. If any observable or macroscopic state of nature is determined, other states become determined. And that is after all the essence of the old mechanics. It was never necessary for physical science to assume that the classical mechanics completely exhausted the nature of things. The geometrical points and lines, the regular spheres, the perfectly rigid bodies of the classical mechanics were generally recognized as limiting concepts which represented, to be sure, features of the actual world but never exhaustive accounts. The new mechanics recognizes this limitation explicitly. It uses a wider conception of "coordinates" or the elements which determine a "state" of nature. Just as a number may be resolved in different ways as the products of two factors, or just as a force acting in any direction may be resolved into different pairs of components, so states of nature according to the new physics can be regarded as resolvable into different "superposed" states without altering the physical consequences. The invariant relations of mechanics bind features of the visible macroscopic states together, and in that respect the classic tradition of mechanics still holds the field, even though we no longer hold the faith in very *simple* laws governing all things from the greatest to the smallest.

III

Our brief glimpse of statistical mechanics reinforces the point that while statistical knowledge is distinct from mechanics it depends upon the latter as well as on history. There is no doubt that we can sometimes make progress in science by ignoring the question of mechanics and studying variations statistically. This has notably been the case with the Darwinian doctrine of natural selection. Neither Darwin nor Mendel went into the causes of variation. Still Mendel, trained as a physicist, made possible the great advance in genetics by seeking and finding an invariant rate. There is no reason to suppose that if variations were completely lawless, the resultant of a large number of them would show any order or recognizable pattern. If any group shows conformity to the law of large numbers, it means that its individual variations are governed by certain forces which bring about symmetry, decreasing frequencies of larger variations, and all the other features which have to be assumed in order to justify a frequency curve. This brings us to the main issue.

The statistician used to be taught to have faith in determinism.

Shall he now follow popularizers of recent physics and regard it more scientific and up-to-date to profess indeterminism? It is tempting to say that he need take neither position, that all he needs to assume is that the universe has certain observable statistical regularities. What, however, does the latter statement imply? Admitting for example, that we do not know the cause or causes which disintegrate any particular atom of uranium, and that all we do know are observable regularities in the behavior of the macroscopically visible masses, the fact remains that we cannot get along in science or in practice without expecting such regularities to recur. What ground have we for this expectation? The answer is that if we assume that our visible material is composed of an unimaginably large, but still calculable, number of invisible microscopic elements and that there are invariant laws according to which the happenings in this realm may be distributed and integrated, we have the most powerful means at our command for the discovery and understanding of an ever increasing number of physical phenomena. We may therefore regard our assumption as well established or verified as any other bit of reliable knowledge in our possession. If the most reliable social statistics, such as our mortality rates for different years, do not exhibit such measurable stability, it is not merely due to the smaller numbers in the latter field, but also to the fact that we here deal with more factors that are not isolable, so that the invariant relations between them are not so readily ascertained. Hence more stable uniformities in the social realms are to be found not only by increasing the number of observations, but also by greater refinement of analysis. And this is exactly what a life insurance company does when instead of relying merely on an ever larger number of death records, it takes advantage of medical research as to the *causes* which prolong or shorten life. Its business would be even more secure if it could determine the true causes of war and rebellion as well as earthquakes and plagues. Without causal laws for the recurrence of phenomena, all our observations would at best belong only to the field of history and not to natural science. We should put down in our books that these frequencies were observed at such and such times; but we shall have no reason for expecting them to recur unless we believed such recurrence to be grounded in the nature of things.

A world of which statistical knowledge is possible is then a world in which there are variations which are in part individually unforeseeable and unpredictable, and therefore, inexplicable, but in which certain laws or regularities prevail. And if some one were to maintain as Poincaré once suggested, that the laws of nature are themselves changing then we should look for the law according to which such change

takes place. A body changes its position according to some law which may be expressed as a constant velocity. So changes in velocity involve either a constant acceleration or one that changes according to some law of force. Ultimately all change involves some constancy. This is not an empirical observation merely, but a condition of significant discourse. We cannot talk sense except on condition that our expressions have some definite meaning and there is no meaning without some element of constancy in the field of variation, wherever that is chosen.

We may see the same truth in a more specifically statistical context. Traditional statistical procedure has always assumed that the variations in our measurements of such magnitudes as the height of a mountain or the specific gravity of iron, are not variations of the object itself which is supposed to be constant, but are rather due to changes in our manipulations of the instruments of measurement. Now, suppose one questions this assumption, as Charles Peirce did long before the rise of the Indeterminacy Principle in physics. That highly original American philosopher ventured to suggest that behind the variations which fall within the limits of the error of measurement there are still smaller variations of the object itself, so that absolute constancy is an ideal never attained in actuality. As a philosophic suggestion this is worthy of more attention than we can devote to it here. For our present purpose it is sufficient to note that so long as the *variation can be expressed in some laws*, we have sufficient determinism for a statistical universe.

The recognition of the view that constancy and variation are polar categories, opposed but inseparable, each meaningless apart from the other, enables us to deal more clearly with the vexed problem as to chance and determinism, neither of which can be eliminated from a statistical view of nature. It is popular to say that chance merely expresses our ignorance. But this offers no basis for mathematical computation. Measurable chance is something that certainly does not depend on *individual* ignorance. When I ask today what chance is there of my dying on a Tuesday, there is to be sure an element of ignorance involved; but that is the ignorance which is necessarily involved in any system of finite knowledge. Nothing that anyone can find out by medical research or otherwise will be sufficient to determine completely or absolutely the individual event—not even the fact that I am condemned by a court of law to be hanged on that day. But on the assumption that the day of the week makes no difference, that as many people die on Tuesdays as on any other day of the week, and that we have to die on one or other of these, the chance of its happening on a Tuesday is one-seventh. The ignorance involved here is not absolute. We do

know something about the causes of death which enables us not only to be certain that death is coming, but that all pretensions that Tuesday is a lucky day or an unlucky one rest on insufficient evidence. If, on the other hand, it is a fact that people generally rest on Sunday or use their automobiles more, this may affect the relative death rates on the different days of the week. We may then say that a chance event dealt with in statistics is one for which we have determining principles that are necessary but insufficient. Pure chance is, indeed, incomprehensible unless defined with reference to some rule determining all possible variations. Thus, if we speak of the chances of a certain die turning up a six in three successive throws, we assume a limited number of possibilities all equally probable. If the chances were not regulated by some rule or order we should have no basis for any one result rather than any other. If there is any reason for any expectation, it is because there is some determination in the case, even though we do not know enough of what determines the individual event.

Similarly, reflection on the abstract character of the repeatable patterns which are the subject of statistical as well as of mechanical laws, enables us to see that law is meaningless apart from all contingency or chance.

It is a great mistake to believe that mechanical science involves a completely determined world, such as the Hegelian Absolute. On the contrary, if there are repeatable patterns or laws, there must be genuine plurality and this necessarily means relative independence or indifference. Thus, if everything depended on everything else, there would be no sense in saying that the photo-electric effect depends on the frequency of the impinging light, and not on other factors. Chance and determination are thus both objective logical consequences of the fact that any system with which science can deal must be defined and limited. Consider, for instance, the laws of celestial mechanics which make our solar system determinate. That some powerful body shall pass it so near as to cause it all to be smashed, would be an accident in the sense that from the coordinates of our system and the laws of celestial mechanics, the event cannot be determined. We can of course insist that if we knew the distance between our solar system and that body, and knew the rates at which they approach each other, we could predict the solar catastrophe. But this only means that we have to enlarge our original system to explain an event which would be an accident within the narrower domain. If, then, we distinguish between relativity and subjectivity, accidents are relative to the system and not to mere subjective opinions.[3]

[3] Professor Ducasse's strictures (page 347) ignore this relativity. The determinism of an absolutely complete universe is not an attainable,—certainly not a verifiable—factor in any natural science.

The question, however, may be raised, Are there any isolated systems? Are not all things inter-connected? In reply, we must insist that any intelligible system with which science can deal must be defined by a limited number of laws and determination thus limited. If you look at the world geometrically, certain features are connected with other features in certain invariant relations. If a disk, for instance, is circular or triangular, we compute its area according to certain invariant consequences of its geometric properties. That, however, which is necessary to determine an object geometrically will not be sufficient mechanically. So likewise, the masses and motion of a system may not determine all of its physical properties, and the latter may not be sufficient to specify the properties of an organism. Similarly, biologic factors alone may not be sufficient to determine how a man will vote or respond to a given argument. In general, sciences aim to attain the form of logical systems in which a limited number of laws determine all the processes and connections between the variables in the field. The propositions of any science thus form what a mathematician calls a group. To the extent that a number of propositions can be derived from a certain number of assumptions, they are so far independent of the truth or falsity of other propositions.

The foregoing considerations do not deny that as science grows, we learn more about the determination of things. We can see this in the way science finds univocal relations to take the place of less determinate or one-to-many relations. Consider a case of what is called a plurality of causes. If I think of headaches as a single species, I may find the causes in eye-strain, in noxious smoke, in improper food, in the degeneration of certain tissues or other factor. This may be sufficiently determinate for practical purposes. However, for scientific purposes I may analyze the different kinds of headaches and find that the one due to eye-strain is different from the one due to alcoholic drink. Or again, I may find that in all these different forms of headaches, there is one common factor, let us say a certain pressure on certain nerve endings. In the end, however, the determinism aimed at in any science is abstract. While events in nature are individualized, and every moment is different from every other, mechanical science and statistics are concerned with the fungible aspects of things, with those features which repeat themselves indefinitely. William James somewhere refers to an animal that is being experimented upon and referred to as a specimen, saying if it could, " Excuse me, it's me." We may stretch this and say that if the individual molecule could speak, it would say, "To be sure, I am a member of the noble species of hydrogen molecules, but I am altogether different from some of my degenerate neighbors." Physics may recognize different *kinds* of hydrogen atoms,

just as vital statistics may distinguish between male and female deaths. It is to history and to intimate human relations that individual differences are of the utmost importance. The individual is more than any of its phases, and there would be no determination if there were not something to be determined. If, therefore, there is such a thing as genuine individuality, if the world consists of a number of things which are genuinely distinct from each other, although they modify each other in certain abstract ways, then there is a genuine incommensurability between the individual and the universal. Each is an unattainable limit to the other. No number of rules then can exhaustively determine the fullness of individual existence, though every change of abstract or isolated phase may be invariably connected with corresponding changes in other beings. Statistics and mechanics cannot therefore dispense with history.

IV

The foregoing reflections are not without bearing on the problem of measurement by statistical methods in the social sciences.

In the first place, we ought not to overlook the obvious distinction between enumeration and measurement. We can enumerate a multitude, but the measurement of a continuous quantity or magnitude involves other operations. Thus we measure length by laying off a standard unit. We measure weight by a process of balancing, and of course the measurement of all derivative magnitudes involves more complicated processes. The full meaning of measurement in the physical sciences is seen when that which is measured is an extensive quantity to which the addition theorem is applicable. Those traits of nature which are not subject to these conditions are not, strictly speaking, measurable. Briefly, the modern analysis of measurement calls attention to the following conditions: (1) we must be able to identify a standard character or state capable of indefinite repetition, (2) we must define some operation to determine when two magnitudes are equal, and (3) we must define some operation which will give meaning to the sum of two or more magnitudes. It may be urged that in the measurement of intensive magnitudes, the last of these conditions is absent. The fact, however, is that in the measurement of physical intensities such as temperature and the like, we always try to connect that which is measured with extensive elements such as distances on a thermometer scale, and this is very difficult to achieve in the social realm. Even if we limit ourselves to intensive social magnitudes such as pleasures or preferences, we find that statistics do not give us very much information, for our human preferences are proverbially inconsistent or

highly variable in time according to incalculable subjective factors. We may put it in other terms by saying that in measuring human attitudes by questionnaires or records of preferences, we are dealing with the resultants of so many factors that the empirical results are seldom highly significant. For, as we saw before, where we do not isolate a single determinant we cannot expect to find a determinate relation. Somewhat similar warnings have been made in biology and in physics; and social phenomena are even more complicated because they include not only physical and biologic elements, but also such factors as fashion, linguistic attitudes and the like. It becomes evident, then, that mere accumulation of social statistics will not enable us to isolate the relevant factors and to determine constant relations any more than we have been able to do with meteorologic statistics. Where we deal with fungible goods as we do in the realm of economics, we ignore all but the factor defined, such as price, amount of supply, etc. But how strong a desire for economic gain will outweigh tabus against forbidden food, working on the Sabbath, making graven images, or doing something which is dishonorable or results in loss of caste, we are hardly in a position to tell by statistical methods alone. Certainly the questionnaire method with children in the movies or students in the class room, selected because they can be readily induced to take the trouble to answer, can hardly claim to be adequate measurement. There is little evidence that the replies of other people will be the same as that of our selected group, and there is no way in which we can be assured that we have not fallen into the fallacy of selection no matter how much care we have exercised to choose at random. There are always characteristics of our group which we do not have in mind or reject as irrelevant but which may in fact be the most important factors in determining the results—for instance, our particular neighborhood, social class, temporary fashion or response to some special condition of the experiment which we have no thought of as significant. Thus, some one examines a number of school children in a given city and compares the standing of Jews and non-Jews. But the Jews happen to be of a special neighborhood where the parents follow certain occupations or all come from Turkey or Saloniki, and are as different from Russian or German Jews as Turks are from Russians and Germans.

Another confusion in the attempt to use statistics in social measurement is the failure to discriminate between a phenomenon and people's opinion about it. Suppose you wish to measure that illusive quality, intellectual eminence. A leading psychologist who is entitled to our profound respect because of his many achievements, tries to dispose of the issue by asking a number of specialists to make an estimate and

then treating their answers statistically. But is not this measuring rather a group of opinions? The belief that the average of a number of judgments by experts is necessarily more correct than the judgment of any one of them is based on the assumption that all these judgments are independent. But is that assumption always true? Experts in a given field read the same books or periodicals and are subject to vogues or fashions. When a vote is taken as to who is the leading psychologist, ablest baseball player, most beautiful actress or most interesting novel, the results may be more indicative of something about the voters for the time being than about that which is voted.

The whole scheme of intelligence-testing assumes something whose nature is very ill-defined. Indeed, some of the leaders in this field first took the trouble to prove that there is no such thing as general intelligence and then proceeded to devise methods for measuring it. We must have clear ideas as to what it is that we are measuring.

There are other and subtler fallacies resulting from too great reliance on the technical procedures of statistical methods. Consider, for instance, some of the conclusions as to heredity by Galton and Karl Pearson. If we ignore the statistical methods and look at the matter critically, we can see that the extent of their material does not justify their conclusions. Surely a study of a thousand cases for two or three generations cannot determine the laws of inheritance, as to what traits are and what traits are not constant in environmental changes. I do not wish to deny that improved statistical methods enable us to guard against such errors. But it is elementary logic rather than technical statistics that enables us to realize this. I may in passing express the suspicion that biometric methods in genetics may by emphasizing unit characters, be doing violence to the fact that in an organism various elements are not completely independent, and that statistical methods based on the assumption of such independence lead us astray.

BIBLIOGRAPHIC NOTE

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DISCUSSION

1. In Professor Cohen's paper occurs the statement that "accidents are relative to the system and not to our subjective opinions." This means that an accident in a given system is that which cannot be predicted in terms of the laws of that system and the state of the system at a given time. It seems to me, however, that "a given system" means only such a portion of the universe as *we* choose to consider; and that the fact that accidents do occur in a system we consider shows that, objectively, it is not properly called *a* system, but is only a part of the total system of the universe, and connected with its other parts. The accidentality of accidents is thus wholly relative to subjective selection by us of *a part only* of the universe.

2. Professor Cohen objects to the statement that "chance merely expresses our ignorance" on the ground that, e.g., the chance of my dying on a Tuesday can be calculated, and ignorance is not a basis for calculation.

Is it not the fact, however, that the word "chance" is currently used in two distinct, though related, ways? In the expression "the chance of my dying on a Tuesday," chance means *probability*, and that probability is calculated on the basis of what we *now do know* about the day of the week on which my death will occur, viz. that it is one out of only seven.

In an expression such as: "whether so and so will occur is a matter of chance," a matter of chance means a matter *unpredictable* because we *do not know* what would be needed to predict it. Thus, the word chance is used sometimes to refer to the fact that we *know* enough to tell something (though not everything) that we should like to be able to tell about the occurrence of an event; and sometimes to refer to the fact that we *do not know* enough to tell anything that we should like to be able to tell about the occurrence of an event.

3. Professor Cohen stated that, with the growth of our knowledge, "events which are undetermined now, will become determined later." This seems to me to overlook the difference between determinism and predictability. Predictability requires not only that events be determined, i.e., be according to some law, but in addition that we should (1) know what that law is, and (2) know what now is the state of the system within which that law holds. Growth of knowledge, thus, may render the now unpredictable later predictable, but not the now undetermined later determined.

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EVALUATING THE EFFECT OF INADEQUATELY MEASURED VARIABLES IN PARTIAL CORRELATION ANALYSIS

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Partial and multiple correlation are used, ordinarily, in the absence of a theory as to the mathematical relationship among the variables. A simple linear combination is assumed and the principal attention is focused either on the regression equation as a predictive tool, on one of the partial correlation coefficients, or on a comparison of the so-called "relative importance" of the different independent variables.

It is not generally recognized that such an analysis assumes that each of the variables is perfectly measured, such that a second measure X'_i, of the variable measured by X_i, has a correlation of unity with X_i. If some of the measures are more accurate than others, the analysis is impaired. For example, the sociologist may have a problem in which an index of economic status and an index of nativity are independent variables. What is the effect, if the index of economic status is much less satisfactory than the index of nativity? Ordinarily, the effect will be to underestimate the significance of the less adequately measured variable and to overestimate the significance of the more adequately measured variable.

A variable may be "inadequately" measured in either or both of at least two respects: (1) The measure may have low reliability, that is, it fails to measure *something* consistently. For example, a score derived from the odd-numbered questions on a test of social attitudes may have a low correlation with the parallel score derived from the even-numbered questions on the same test. Or, the schedules in a standards-of-living study may be so badly filled out that the correlation between indexes derived from similar schedules filled out by two different interviewers of the same families may be low. (2) The measure may have high reliability, yet low validity. That is, it fails to measure adequately what it purports to measure. A reliable test may not necessarily be a valid test of social attitudes, as might be checked by correlating the test scores with some other index of social attitudes. Or, indexes derived from accurately filled out schedules in a standards-of-living study may have a low correlation with indexes from schedules based on a

different, though equally defensible, concept of standards of living.[1]

If either the reliability or validity of an index is in question, at least two measures of the variable are required to permit an evaluation. The purpose of this paper is to provide a logical basis and a simple arithmetical procedure (a) for measuring the effect of the use of two indexes, each of one or more variables, in partial and multiple correlation analysis and (b) for estimating the likely effect if two indexes, not available, could be secured.

THEORETICAL CONSIDERATIONS

Let us assume that we have s variables, of each of which there exist two measures X and X', based on n cases. Our problem is to compare the results from the use of both X_i and X'_i with the results from the use of X_i alone. The problem might be examined still more generally by considering k measures of each variable X_i. The present paper, however, is limited to a consideration of the two measures X_i and X'_i, of each of our s variables.

The writer has considered three different approaches, which, though different in their initial logic, lead, as will be proved, to identical results in important special cases.

(1) If we consider X_1, the dependent variable, satisfactorily measured, such that X'_1 may be disregarded, we may find the multiple correlation of X_1 with X_2 and X'_2 holding constant the remaining $2(s-2)$ variables. The theory was described by the writer in a previous paper in this JOURNAL.[2] Expressing v_1, v_2, and v'_2 as respective deviations from the planes

$$v_1 = X_1 - (a_1 + b_{13.3'4\ldots ss'}X_3 + b_{13'.34\ldots ss'}X'_3 + \cdots + b_{1s'.33'\ldots(s-1)'s}X'_s)$$

$$v_2 = X_2 - (a_2 + b_{23.3'4\ldots ss'}X_3 + b_{23'.34\ldots ss'}X'_3 + \cdots + b_{2s'.33'\ldots(s-1)'s}X'_s)$$

$$v'_2 = X'_2 - (a'_2 + b_{2'3.3'4\ldots ss'}X_3 + b_{2'3'.34\ldots ss'}X'_3 + \cdots + b_{2's'.33'\ldots(s-1)'s}X'_s)$$

one finds the multiple correlation between the values of v. This correlation coefficient, since it has properties both of multiple and of partial correlation, has been called $r_{1.22'.33'\ldots ss'}$, the coefficient of combined partial correlation, and it has been shown that it may be expressed in terms of conventional values of r by writing

$$r_{1.22'.33'\ldots ss'} = \sqrt{1 - (1 - r^2_{12.33'\ldots ss'})(1 - r^2_{12'.233'\ldots ss'})}. \qquad (1)$$

[1] Another type of inadequacy may arise when X_1 and X_2, say, are ratios with a common inaccurate denominator p, while X_3 does not contain p. Then r_{12} under certain conditions will be too high. (Cf. Karl Pearson, *Proceedings of the Royal Society*, lx, 1897, p. 489.) If r_{12} is too high, $r_{12.3}$ ordinarily will be overestimated as compared with $r_{12.3}$. However, this so-called "spurious correlation" is negligible in cases where it is logical to use percentages or ratios in social or demographic statistics, as G. Udny Yule has shown (*Journal of the Royal Statistical Society*, lxxiii, 1910, p. 644). The writer's present considerations do not deal with the problem discussed by Pearson and Yule and should not be confused with it.

[2] "A Coefficient of 'Combined Partial Correlation' with an Example from Sociological Data," this JOURNAL, v. 29, March, 1934, pp. 70–71.

This approach to our problem has two limitations, among others. It does not provide for two measures of the dependent variable and it requires $(2s-1)$ dimensions for handling a problem with only s sets of measures. On the other hand, if one has a problem with only three sets of measures this method provides a useful procedure for comparing $r_{1.22'.33'}$ with $r_{1.33'.22'}$ and noting how they differ from $r_{12.3}$ and $r_{13.2}$, respectively. Equation 1 may be written

$$r_{1.22'.33'} = \sqrt{\frac{r^2_{12.33'} + r^2_{12'.33'} - 2r_{12.33'}r_{12'.33'}r_{22'.33'}}{1 - r^2_{22'.33'}}}. \tag{1a}$$

If $r_{12}=r_{12'}$, and $r_{23}=r_{2'3}=r_{23'}=r_{2'3'}$, while $r_{22'}$ and $r_{33'}$ are each $\neq \pm 1$, Equation 1a reduces to a simple form in terms of zero-order r's, namely,

$$r_{1.22'.33'} = \frac{r_{12}d_{33} - r_{13}r_{23}}{\sqrt{(d_{33} - r^2_{13})(d_{22}d_{33} - r^2_{23})}}, \tag{1b}$$

where $d_{ii}=\frac{1}{2}(1+r_{ii'})$. If we possess only one index of the variable measured by X_3, Equation 1a reduces to

$$r_{1.22'.3} = \sqrt{\frac{r^2_{12.3} + r^2_{12'.3} - 2r_{12.3}r_{12'.3}r_{22'.3}}{1 - r^2_{22'.3}}}, \tag{1c}$$

which, if $r_{12}=r_{12'}$, and $r_{23}=r_{2'3}$, while $r_{22'}\neq \pm 1$, reduces to the very convenient form

$$r_{1.22'.3} = \lambda r_{12.3}, \tag{1d}$$

where $\lambda=\sqrt{(1-r^2_{23})/(d_{22}-r^2_{23})}$ and $d_{22}=\frac{1}{2}(1+r_{22'})$. It is evident from an inspection of the expression under the radical that $r_{1.22'.3}>r_{12.3}$, as would follow from the property of $r_{1.22'.3}$ as a multiple correlation coefficient. The value of $r_{1.22'.3}$ may be compared with the conventional value of $r_{13.22'}$ and it can then be noted how they differ from $r_{12.3}$ and $r_{13.2}$, respectively.

(2) Let us now avail ourselves of X_1 and X'_1, two measures of the dependent variable, and join a fourth equation to the three considered above, namely,

$$v'_1 = X'_1 - (a'_1 + b_{1'3.3'4\ldots ss'}X_3 + b_{1'3'.34\ldots ss'}X'_3 + \cdots + b_{1's'.33'\ldots(s-1)'s}X'_s).$$

Write $y_i=v_i/\sigma_{v_i}$ and form the sums $(y_1+y'_1)$ and $(y_2+y'_2)$. Since $\sigma_{y_i}=1$, whence

$$\sigma_{y_i+y'_i} = \sqrt{\sigma^2_{y_i} + 2r\sigma_{y_i}\sigma_{y'_i} + \sigma^2_{y'_i}} = \sqrt{2(1 + r_{y_iy'_i})}$$

and since $\Sigma y_iy_{i'}/n = r_{y_iy'_{i'}}$ we have

$$r_{(y_1+y'_1)(y_2+y'_2)} = \frac{\Sigma(y_1 + y'_1)(y_2 + y'_2)}{n\sigma_{y_1+y'_1}\sigma_{y_2+y'_2}} = \frac{r_{y_1y_2} + r_{y_1y'_2} + r_{y'_1y_2} + r_{y'_1y'_2}}{2\sqrt{(1 + r_{y_1y'_1})(1 + r_{y_2y'_2})}}$$

whence, from the relation $r_{y_iy_j} = r_{ij.33'\ldots ss'}$,

$$r_{(y_1+y'_1)(y_2+y'_2)} = \frac{r_{12.33'\ldots ss} + r_{12'.33'\ldots ss'} + r_{1'2.33'\ldots ss'} + r_{1'2'.33'\ldots ss'}}{2\sqrt{(1 + r_{11'.33'\ldots ss'})(1 + r_{22'.33'\ldots ss'})}}. \quad (2)$$

It is interesting to consider again the case of three variables. Equation 2 reduces to

$$r_{(y_1+y'_1)(y_2+y'_2)} = \frac{r_{12.33'} + r_{12'.33'} + r_{1'2.33'} + r_{1'2'.33'}}{2\sqrt{(1 + r_{11'.33'})(1 + r_{22'.33'})}}. \quad (2a)$$

If we now assume that $r_{ij} = r_{ij'} = r_{i'j} = r_{i'j'}$, while $r_{ii'} \neq 1$, Equation 2a may be shown to reduce to

$$r_{(y_1+y'_1)(y_2+y'_2)} = \frac{r_{12}d_{33} - r_{13}r_{23}}{\sqrt{(d_{11}d_{33} - r^2_{13})(d_{22}d_{33} - r^2_{23})}} \quad (2b)$$

where $d_{ii} = \frac{1}{2}(1+r_{ii'})$. If we use only one index of the independent variable, Equation 2b reduces to a form identical with (1b). Moreover, if we use only one index each of X_1 and X_3, Equation 2b reduces to

$$r_{y_1(y_2+y'_2)} = \lambda r_{12.3}, \quad (2c)$$

which is identical with (1d).

It will be observed that, although $(y_1+y'_1)$ and $(y_2+y'_2)$ are index numbers formed by combining two measures of each factor, it is not necessary arithmetically to go through the process described, since Equation 2 expresses our results directly in terms of correlation coefficients between the original measures of X. The principal limitation of this approach seems to be the fact that it requires $2s$ dimensions to handle a problem involving only s pairs of measures.

(3) By our third approach we reduce the problem to s dimensions. Writing $z_i = (X_i - \overline{X}_i)/\sigma_i$, where $\overline{X} = \Sigma X_i/n$, we form the sums

$$\begin{aligned} t_1 &= z_1 + z'_1 \\ t_2 &= z_2 + z'_2 \\ &\cdots \\ t_s &= z_s + z'_s \end{aligned}$$

and seek to express the relationships among the values of t in terms of relationships among the original values of X.

Remembering that $\sigma_{z_i} = 1$, whence $\sigma_{t_i} = \sqrt{2(1+r_{ii'})}$, and that $\Sigma z_i z_j/n = r_{ij}$, where r_{ij} is the zero-order correlation between X_i and X_j, we have

$$\begin{aligned} r_{t_it_j} &= \frac{\Sigma(z_i + z'_i)(z_j + z'_j)}{2n\sqrt{(1 + r_{ii'})(1 + r_{jj'})}} \\ &= \frac{r_{ij} + r_{i'j} + r_{ij'} + r_{i'j'}}{4\sqrt{[(1 + r_{ii'})/2][(1 + v_{jj'})/2]}} = \frac{\bar{r}_{ij}}{\sqrt{d_{ii}d_{jj}}}, \end{aligned} \quad (3)$$

where $\bar{r}_{ij}=\frac{1}{4}(r_{ij}+r_{i'j}+r_{ij'}+r_{i'j'})$, the average zero-order intercorrelations, and where $d_{ii}=\frac{1}{2}(1+r_{ii'})$.

Consider now the conventional correlation matrix

$$\Delta = \begin{vmatrix} 1 & r_{t_1t_2} & \cdots & r_{t_1t_s} \\ r_{t_1t_2} & 1 & \cdots & r_{t_2t_s} \\ \cdot & \cdot & \cdots & \cdot \\ r_{t_1t_s} & r_{t_2t_s} & \cdots & 1 \end{vmatrix}, \tag{3a}$$

which, upon substitution of the values of $r_{t_it_j}$ found in (3), becomes

$$\Delta = \begin{vmatrix} 1 & \frac{\bar{r}_{12}}{\sqrt{d_{11}d_{12}}} & \cdots & \frac{\bar{r}_{1s}}{\sqrt{d_{11}d_{ss}}} \\ \frac{\bar{r}_{12}}{\sqrt{d_{11}d_{22}}} & 1 & \cdots & \frac{\bar{r}_{2s}}{\sqrt{d_{22}d_{ss}}} \\ \cdot & \cdot & \cdots & \cdot \\ \frac{\bar{r}_{1s}}{\sqrt{d_{11}d_{ss}}} & \frac{\bar{r}_{2s}}{\sqrt{d_{22}d_{ss}}} & \cdots & 1 \end{vmatrix}. \tag{3b}$$

Multiply the elements in the first row by $\sqrt{d_{11}}$, the elements in the second row by $\sqrt{d_{22}}$, etc. Multiply the elements in the first column by $\sqrt{d_{11}}$, the elements in the second column by $\sqrt{d_{22}}$, etc. We have

$$\Delta = \frac{1}{d_{11}d_{22}\cdots d_{ss}} \begin{vmatrix} d_{11} & \bar{r}_{12} & \cdots & \bar{r}_{1s} \\ \bar{r}_{12} & d_{22} & \cdots & \bar{r}_{2s} \\ \cdot & \cdot & \cdots & \cdot \\ \bar{r}_{1s} & \bar{r}_{2s} & \cdots & d_{ss} \end{vmatrix} \tag{3c}$$

and we write

$$\Delta = \frac{\Delta'}{d_{11}d_{22}\cdots d_{ss}}.$$

As a consequence of the operation in passing from (3b) to (3c), any $(s-1)$-rowed minor of Δ',

$$\Delta'_{ij} = \frac{\sqrt{d_{ii}d_{jj}}}{d_{11}d_{22}\cdots d_{ss}}\Delta_{ij}, \tag{3d}$$

where Δ'_{ij} is formed by crossing out the i'th row and j'th column in Δ'. This is a special case, where $m_i=2$, of a more general determinant in which $d_{ii}=[1+(m_i-1)\bar{r}_{ii}]/m_i$, in which $\bar{r}_{ii}$ is the average of the $m_i(m_i-1)/2$ intercorrelations between m_i measures of a given varia-

ble i. It is of interest to note that as $m_i \to \infty$, $d_{ii} \to \bar{r}_{ii}$, whence, if m_j also $\to \infty$, $\bar{r}_{ij}/\sqrt{d_{ii}d_{jj}} \to \bar{r}_{ij}/\sqrt{\bar{r}_{ii}\bar{r}_{jj}}$, which is a form of the correlation coefficient corrected for attenuation.[3]

Solution of this determinant gives the equations needed, in terms of zero-order correlation coefficients, for the complete analysis of our problem. Equation 3c makes explicit the assumption, as to the intrinsic accuracy of all variables, which is implicit in the conventional partial correlation analysis. Only as every value $r_{ii'} \to 1$, whence $d_{ii} = \frac{1}{2}(1+r_{ii'}) \to 1$, and as every value $\bar{r}_{ij} \to r_{ij}$, does Δ' approach the usual form. In other words, the customary correlation analysis assumes that every X_i would correlate perfectly with another measure X'_i, of the same variable.

Let us now consider three variables only, namely, t_1, t_2, and t_3. Equation 3c becomes

$$\Delta = \frac{1}{d_{11}d_{22}d_{33}} \begin{vmatrix} d_{11} & \bar{r}_{12} & \bar{r}_{13} \\ \bar{r}_{12} & d_{22} & \bar{r}_{23} \\ \bar{r}_{13} & \bar{r}_{23} & d_{33} \end{vmatrix}. \tag{3e}$$

The four types of values in which there is likely to be most interest with respect to our present problem are $r_{t_1t_2 \cdot t_3}$, $\beta_{t_1t_2 \cdot t_3}$, $\beta_{t_1t_2 \cdot t_3}r_{t_1t_2}$, and $R^2_{t_1 \cdot t_2t_3}$. In the interest of clarity, the notation will be changed by writing $(i+i)$ in the place of t_i in the subscripts. Let us now express our desired measures in terms of the zero-order correlations between the original values of X_1, X_2, and X_3, remembering that $\bar{r}_{ij} = \frac{1}{4}(r_{ij}+r_{ij'}+r_{i'j}+r_{i'j'})$, and that $d_{ii} = \frac{1}{2}(1+r_{ii'})$.

(a) *When there are two measures each of X_1, X_2, and X_3.*

$$\begin{aligned} r_{(1+1)(2+2)\cdot(3+3)} &= \frac{\Delta_{12}}{\sqrt{\Delta_{22}\Delta_{11}}} = \frac{\Delta'_{12}}{\sqrt{\Delta'_{22}\Delta'_{11}}}, \quad \text{from (3d)}, \\ &= \frac{\bar{r}_{12}d_{33} - \bar{r}_{13}\bar{r}_{23}}{\sqrt{(d_{11}d_{33} - \bar{r}^2_{13})(d_{22}d_{33} - \bar{r}^2_{23})}}. \end{aligned} \tag{3f}$$

It will be observed that if we write $r_{ij} = \bar{r}_{ij}$, (3f) becomes identical with (2b); otherwise, (3f) may be expected to differ from (2) because of the different logic behind the respective derivations.

$$\begin{aligned} \beta_{(1+1)(2+2)\cdot(3+3)} &= \frac{\Delta_{12}}{\Delta_{11}} = , \qquad \text{from (3d)}, \\ \frac{\Delta'_{12}}{\Delta'_{11}}\sqrt{\frac{d_{22}}{d_{11}}} &= \left(\frac{\bar{r}_{12}d_{33} - \bar{r}_{13}\bar{r}_{23}}{d_{22}d_{33} - \bar{r}^2_{23}}\right)\sqrt{\frac{d_{22}}{d_{11}}}. \end{aligned} \tag{3g}$$

[3] For the general proof see the writer's paper, "Reliability Coefficients in a Correlation Matrix," *Psychometrica*, June, 1936. Equation 3 can be shown to be a special case of Equation 147 in Truman L. Kelley, *Statistical Methods*, p. 197.

$$\beta_{(1+1)(2+2).(3+3)}r_{(1+1)(2+2)} = \left[\frac{\bar{r}_{12}d_{33} - \bar{r}_{13}\bar{r}_{23}}{d_{11}(d_{22}d_{33} - \bar{r}^2_{23})}\right]\bar{r}_{12}. \tag{3h}$$

$$R^2_{(1+1).(2+2)(3+3)} = 1 - \frac{\Delta}{\Delta_{11}} = 1 - \frac{\Delta'}{\Delta'_{11}d_{11}} = \frac{\bar{r}^2_{12}d_{33} + \bar{r}^2_{13}d_{22} - 2\bar{r}_{12}\bar{r}_{13}\bar{r}_{23}}{d_{11}(d_{22}d_{33} - \bar{r}^2_{23})}. \tag{3i}$$

(b) *When there are two measures each of X_1, and X_2, and when there is one measure of X_3.* Substitute $d_{33}=1$ in Equation 3e or Equations 3f to 3i, inclusive. Example:

$$r_{(1+1)(2+2).3} = \frac{\bar{r}_{12} - \bar{r}_{13}\bar{r}_{23}}{\sqrt{(d_{11} - \bar{r}^2_{13})(d_{11}d_{22} - \bar{r}^2_{23})}}. \tag{3j}$$

If we write $r_{ij}=\bar{r}_{ij}$, (3j) becomes identical with results obtained from reducing (2b).

(c) *When there is one measure of X_1 and when there are two measures each of X_2 and X_3.* Substitute $d_{11}=1$ in (3e) or Equations 3f to 3i, inclusive. Example:

$$r_{1(2+2).(3+3)} = \frac{\bar{r}_{12}d_{33} - \bar{r}_{13}\bar{r}_{23}}{\sqrt{(d_{33} - \bar{r}^2_{13})(d_{22}d_{33} - \bar{r}^2_{23})}}. \tag{3k}$$

If we write $r_{ij}=\bar{r}_{ij}$, Equation 3k becomes identical with (1b), or with (2b) when $d_{11}=1$. The three approaches to our problem coincide in results at this point, although, if $r_{ij}\neq\bar{r}_{ij}$, we may expect differences.

(d) *When there is one measure each of X_1, and X_3, while there are two measures of X_2.* Substitute $d_{11}=d_{33}=1$ in Equation 3e or Equations 3f to 3i, inclusive. Write $r_{13}=\bar{r}_{13}$. Examples:

$$r_{1(2+2).3} = \frac{\bar{r}_{12} - \bar{r}_{13}\bar{r}_{23}}{\sqrt{(1 - r^2_{13})(d_{22} - \bar{r}^2_{23})}}, \tag{3l}$$

which, if $r_{12}=r_{12'}$ and if $r_{23}=r_{2'3}$, reduces, exactly as (1b) and (2b) reduce, to

$$r_{1(2+2).3} = \lambda r_{12.3}, \tag{3m}$$

where $\lambda=\sqrt{(1-r^2_{23})/(d_{22}-r^2_{23})}$, an identity with (1d) or (2c). Moreover,

$$r_{13.(2+2)} = \frac{r_{13}d_{22} - \bar{r}_{12}\bar{r}_{23}}{\sqrt{(1 - r^2_{13})(d_{22} - \bar{r}^2_{23})}}. \tag{3n}$$

If $r_{12}=r_{12'}$ and if $r_{23}=r_{2'3}$, we may write (3n) as

$$r_{13.(2+2)} = \frac{r_{13}d_{22} - r_{12}r_{23}}{\sqrt{(1 - r^2_{13})(d_{22} - r^2_{23})}}, \tag{3o}$$

which can be shown to be identical with the conventional formula for $r_{13.22'}$ under the same assumptions, where $r_{13.22'}$ is the partial correlation coefficient between X_1 and X_3, with X_2 and X'_2 held constant. When $r_{12}=r_{12'}$ and $r_{23}=r_{2'3}$ and when $r_{12.3}$, $r_{13.2}$, $r_{1(2+2).3}$, and $r_{12.(3+3)}$ each $\neq 0$, we write

$$\frac{r_{1(2+2).3}}{r_{13.(2+2)}} = k\frac{r_{12.3}}{r_{13.2}}, \qquad \text{whence}$$

$$k = \frac{\lambda r_{13.2}}{r_{13.(2+2)}} = \frac{r_{13} - r_{12}r_{23}}{r_{13}d_{22} - r_{12}r_{23}} > 1,$$

if r_{13} is positive and $>r_{12}r_{23}$, or if r_{13} is negative and $<r_{12}r_{23}$.

If $r_{12}\neq r_{12'}$ and if $r_{23}\neq r_{2'3}$, the logic of our derivation would require that $r_{1(2+2).3}$ be compared with $r_{13.(2+2)}$, rather than with $r_{13.22'}$. It will be observed that the arithmetical operations needed to calculate (3n) are simpler than those needed to calculate $r_{13.22'}$.

By similar methods the reader may find easily the values of $r_{(1+1).23}$ or of any other functions derived from the correlation matrix.

We have seen that when $r_{ij}=r_{ij'}=r_{i'j}=r_{i'j'}$, our second and third theoretical approaches lead to identical values of partial r, and that when $r_{11'}=1$, where X_1 is the dependent variable, our first approach also coincides in results. It is the writer's judgment that the third approach is to be preferred, both theoretically and practically, because of its simplicty and generality. It reduces a problem with $2s$ sets of measures to one of s dimensions. It permits a ready comparison not only of such values as $r_{1(2+2).3}$ and $r_{13.(2+2)}$, or some functions thereof, but also of such values as $\beta_{1(2+2).3}$ and $\beta_{13.(2+2)}$, or the products of the Betas with $r_{1(2+2)}$ and r_{13}, respectively, or of such a value as $R^2_{1.(2+2)3}$. It avoids logical difficulties as to dependent and independent variables which might possibly appear from the application of least square theory in the second approach, and it permits the computation of standard errors by conventional formulas. Each of the three approaches assumes X_i and X'_i to be of equal weight or value for use in an index.

The third approach, it will be remembered, assumes that an index number t_i is formed by finding $z_i=(X_i-\overline{X}_i)/\sigma_i$ and $z'_i=(X'_i-\overline{X}'_i)/\sigma'_i$ and adding these two standard measures. It is possible, especially if X is a fraction and X' is another measure of $(1-X)$, that X and X' will be negatively correlated. Naturally, in combining X and X' in an

index, a research worker would reverse the signs either of z or z', making the correlation positive. This is not strictly required in the theoretical development above; except that if $r_{ii'}$ is negative the problem becomes indeterminate when $r_{ii'} = -1$. Arithmetically, of course, it is not necessary to compute the index number t_i, as Equations 3f to 3i, inclusive, or any other measures derived from Equation 3c, may be computed directly from the correlation coefficients involving the original measures of X and X', taken individually. If $r_{ii'}$ is negative, one should change the sign of $r_{ii'}$ to positive and reverse the signs in all other correlation coefficients involving X'.

Finally, it often happens that one has some reason to believe that a particular index is inadequate, yet has no second measure at hand. Nevertheless, he would like to know roughly how much difference it might make in his final interpretation if some second index could have been used. If he is willing to assume that the correlations of his unknown second index with the other variables would be the same as the correlations of his known first index with these variables, he can set an upper and lower limit of discrepancy by arbitrarily assigning to the unknown $r_{ii'}$ a low value and then a high value. In the special case where Equation 3m is applicable, no computation is required, as values of λ in (3m) are presented in Table I for selected values of $r_{22'}$ and r_{32}, or, rather, more generally for selected values of $r_{jj'}$ and r_{jk}. It should be said with emphasis, however, that values derived by making these assumptions never should be reported *in lieu of* $r_{12.3}$ or $r_{13.2}$. The new

TABLE I

VALUES OF $\lambda = \sqrt{(1-r^2_{jk})/(d_{jj}-r^2_{jk})}$ FOR USE IN THE EQUATION $r_{i(j+j').k} = \lambda r_{ij.k}$

[Assuming that $r_{ij} = r_{ij'}$, and that $r_{jk} = r_{j'k}$, and writing $d_{jj} = \frac{1}{2}(1 + r_{jj'})$]

r_{jk}	$r_{jj'} = +.50$	$r_{jj'} = +.60$	$r_{jj'} = +.70$	$r_{jj'} = +.80$	$r_{jj'} = +.90$
.00	1.155	1.118	1.085	1.054	1.026
.05	1.155	1.118	1.085	1.054	1.026
.10	1.157	1.119	1.086	1.055	1.026
.15	1.159	1.121	1.087	1.055	1.027
.20	1.163	1.124	1.089	1.057	1.027
.25	1.168	1.127	1.091	1.058	1.028
.30	1.174	1.132	1.094	1.060	1.029
.35	1.183	1.138	1.098	1.062	1.030
.40	1.193	1.146	1.103	1.065	1.031
.45	1.207	1.155	1.110	1.069	1.033
.50	—	1.168	1.118	1.074	1.035
.55	—	1.184	1.129	1.080	1.038
.60	—	—	1.143	1.089	1.042
.65	—	—	1.162	1.100	1.046
.70	—	—	—	1.115	1.053
.75	—	—	—	1.139	1.063
.80	—	—	—	—	1.078
.85	—	—	—	—	1.104

($r_{i(j+j').k} = \lambda r_{ij.k}$) is Equation 3m in this paper.

values are supplements to the information obtained from $r_{12.3}$ and $r_{13.2}$, not substitutes, and may be used cautiously as guides only. The same caution, of course, does not apply to the use of the more general results when all of the zero-order correlations are known, although in any case, the limitation must be kept in mind that X_i and X'_i are receiving equal weights.

ILLUSTRATIONS OF THE APPLICATION

(1) Suppose that we are interested in the question, "Why do residents of some areas of a large city move their abodes less often than residents of other areas?" We should guess that stability of residence must be closely related to home ownership. We also should guess that stability may be related to the presence of larger than average families who have a good many young children.

Using 1934 data for 651 Chicago census tracts,[4] we have three indexes:

X_1 = percentage of families residing at their present abode at least five years prior to the 1934 census.
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We take X_1 as an index of stability in an area, X_2 as an index of larger than average families, and X_3 as an index of home ownership. Finding $r_{12}=.6475$, $r_{13}=.8501$, and $r_{23}=.6055$, we obtain $r_{12.3}=.317$ and $r_{13.2}=.755$.

Unfortunately, our index of larger than average families is unsatisfactory, because it fails to measure adequately the variable in which we are really interested, namely, the presence of larger than average families who have a good many young children. That is, we are questioning the validity of the index when it is to be used as an index of what we want to measure, because it fails to discriminate between families which may be composed wholly of adults and families which are composed partly of small children. It happens that we know the ratio of children under 5 to women 20 to 44 in each tract. Let us call this ratio X'_2 and introduce it as a fourth variable in a conventional correlation analysis. Since $r_{12'}=.5158$, $r_{22'}=.6646$, and $r_{2'3}=.4283$, we have $r_{12.2'3}=.175$, $r_{12'.23}=.179$, and $r_{13.22'}=.758$. Evidently, both of our family indexes now almost vanish as compared with our index of home ownership. But a moment's reflection will indicate that in the present case $r_{12.2'3}$ and $r_{12'.23}$ have little, if any, realistic meaning.

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What we are really interested in is the combined association of X_2 and X'_2 with X_1, as compared with the association of X_3 with X_1.

We decide to form a new family index, $t_2 = z_2 + z'_2$, where $z_2 = (X_2 - \overline{X}_2)/\sigma_2$ and $z'_2 = (X'_2 - \overline{X}'_2)/\sigma'_2$. The computation of this index would be laborious, however, as there are 651 tracts. We can save the labor and get identical results by simply using our observed zero-order correlation coefficients in Equation 31 of the present paper. The computation takes practically no more time than that leading to first-order partials and, of course, much less time than that leading to second-order partials such as those in the preceding paragraph. We find that $r_{1(2+2).3}$, the correlation between the index of stability and the new and more inclusive family index, holding constant the index of home ownership, is .396, by Equation 3l, while $r_{13.(2+2)}$, the correlation between stability and home ownership, holding constant the new family index, is .776, by Equation 3n. We see that $r_{1(2+2).3}$ is about twenty per cent larger than $r_{12.3}$, while $r_{13.(2+2)}$ (which, in most problems, would be smaller than $r_{13.2}$) is only two per cent larger than $r_{13.2}$.

We have been assuming that our index of stability and our index of home ownership are satisfactory. We recall, however, from a study using 1930 census tract data in Cleveland, Ohio[5] that a correlation of only .85 was found between X_3, the percentage of families owning their own homes and X'_3, the percentage of homes owned per 100 dwellings. A reason for the discrepancy is that if an area contains only two-family dwellings, the maximum home ownership by our index could be only 50 per cent, or if an area contains only four-family dwellings the maximum home ownership could be only 25 per cent. For our Chicago series no values of X'_3 have been computed, though they might be obtained if necessary. In the Cleveland study, p. 217, we see that X_3 and X'_3 correlated about alike with several other social and economic variables, none of which, however, correspond to our X_1, X_2, or X'_2. Assuming that the correlations of X'_3 with X_1, X_2, and X'_2 would be about the same as the respective correlations of X_3 with these variables, and assuming that for Chicago $r_{33'}$ would be .90 at the minimum, because we have observed that $r_{13} = .85$, we can estimate what our results might have been if X'_3 had been combined with X_3 in a new index of home ownership. Little additional computation is required. Setting $d_{11} = 1$, $d_{22} = \frac{1}{2}(1 + r_{22'}) = .8323$, $d_{33} = \frac{1}{2}(1 + r_{33'}) = .95$, $\bar{r}_{12} = \frac{1}{2}(r_{12} + r_{12'}) = .58165$, $r_{13} = .8501$, and $\bar{r}_{23} = \frac{1}{2}(r_{23} + r_{2'3}) = .5169$, we we substitute in Equation 3k of the present paper, obtaining

[5] Henry D. Sheldon, Jr., "Problems in the Statistical Study of Juvenile Delinquency," *Metron*, xii, December, 1934, pp. 201–23.

$r_{1(2+2).(3+3)} = .328$, while $r_{1(3+3).(2+2)} = .800$ is obtained after interchanging transcripts 2 and 3 in the same formula. Thus, the inclusion of a second index of home ownership, provided our assumptions hold, may lower $r_{1(2+2).3}$ about 17 per cent and raise $r_{13.(2+2)}$ about 3 per cent. On the basis of this information, we can decide whether or not it is worth while to work up the actual data for X'_3 and bring X'_3 into the problem formally. We might, indeed, decide to neglect both X'_2 and X'_3, since our last result is closer to the original than the second. But we now have information to guide us in our decision.

For comparative purposes, the values discussed, together with some additional values which may be of interest, are recorded below. (Incidentally, the independent computation of the square of the multiple correlation coefficient by two different formulas may be used, as in the conventional correlation analysis, as an automatic check on the arithmetic used in calculating the partial r's and β's.)

$r_{12.3}$	$=.317$	$r_{1(2+2).3}$	$=.396$	$r_{1(2+2).(3+3)}$	$=.328$, estimated.
$r_{13.2}$	$=.755$	$r_{13.(2+2)}$	$=.776$	$r_{1(3+3).(2+2)}$	$=.800$, estimated.
$\beta_{12.3}r_{12}$	$=.136$	$\beta_{1(2+2).3}r_{1(2+2)}$	$=.166$	$\beta_{1(2+2).(3+3)}r_{1(2+2)}$	$=.126$, estimated.
$\beta_{13.2}r_{13}$	$=.615$	$\beta_{13.(2+2)}r_{13}$	$=.612$	$\beta_{1(3+3).(2+2)}r_{1(3+3)}$	$=.661$, estimated.
$R^2_{1.23}$	$=.751$	$R^2_{1.(2+2)3}$	$=.778$	$R^2_{1.(2+2)(3+3)}$	$=.787$, estimated.

(2) Let us suppose that in the foregoing problem we had reason to feel satisfied with X_1 and X_2. Our information from the Cleveland study leads us to wonder how much our values of $r_{12.3}$ and $r_{13.2}$ would be altered if we improved the index X_3 by combining with it X'_3. Assuming that $r_{13'}$ would equal r_{13} and that $r_{23'}$ would equal r_{23}, and writing $r_{33'} = .90$ on the same grounds as in the second paragraph preceding, we have, from Equation 3m,

$$r_{1(3+3).2} = \lambda r_{13.2}$$

where λ may be found without computation, simply by entering our Table I, with $r_{jk} = r_{32} = .85$ and $r_{jj'} = r_{33'} = .90$. We see that $\lambda = 1.104$, and therefore estimate $r_{1(3+3).2} = 1.104 \times .755 = .83$. To estimate $r_{12.(3+3)}$ on the same assumptions, we need only to substitute our observed r_{12}, r_{13}, and r_{23} and our guessed value of $d_{33} = \frac{1}{2}(1 + r_{33'}) = .95$ in Equation 3o (after an interchange of transcripts in 3o), obtaining $r_{12.(3+3)} = .28$, which is about 10 per cent less than $r_{12.3} = .317$.

(3) Returning again to the Cleveland study, we use a different set of data. We seek the relationship between X_1, the juvenile delinquency rate in 1928–31 by census tracts, X_2, an index of dependency in 1928,

and X_3, the percentage of native whites in the population. Given $r_{12}=.75$, $r_{13}=-.51$, and $r_{23}=.60$, from p. 206, we have $r_{12.3}=.65$. After the study is completed, a parallel index for 1931 becomes available. Call it X'_2. Shall we include it in the study? Assume that we have no knowledge of $r_{12'}$, $r_{22'}$, and $r_{2'3}$. Since $r_{12}=.75$, we are probably justified in assuming that $r_{22'}$ is at least .80. While the dependency rate in 1931 is higher throughout the city than in 1928, we have no *a priori* reason to assume that the relationships between dependency and delinquency and nativity have changed markedly. Entering Table I with $r_{jk}=r_{23}=.60$ and $r_{jj}=r_{22'}=.80$, we find $\lambda=1.089$. Hence, we estimate by Equation 3m, $r_{1(2+2).3}=1.089\times.65=.71$, and conclude that with the use of a more reliable index of dependency $r_{1(2+2).3}$ will lie somewhere between .65 (which is $r_{12.3}$) and .71. In this case, actual data happen to be available, p. 218, namely, $r_{12'}=.77$, $r_{22'}=.90$, and $r_{2'3}=.64$, permitting us to use Equation 3l, from which we calculate $r_{1(2+2).3}=.69$.

It is hoped that this paper will interest research workers sufficiently to encourage further exploration of the theoretical approaches here examined. Further empirical study of the range of safety in the use of the approximation formulas also is desirable. From the standpoint of application if there is a hesitance, because of the time required, to use these or better methods which subsequent students may develop, one can say only that an extra few minutes spent in analyzing one's correlation problem is a trivial amount of time as compared with the time taken to collect or reduce the data.

STATISTICAL METHOD AND INDUSTRY IN GREAT BRITAIN

By Egon S. Pearson

The term statistics is understood in so many different senses that it is important to make clear at the outset of this article with which aspect of the subject I am concerned. It is with that branch of the theory of statistics which is closely associated with the theory of probability. Since in any study of the quality of material which he purchases or manufactures, the industrialist is often compelled to draw inferences regarding the aggregate from a part or sample only, it follows that he is concerned with probability rather than certainty. This is the case whether he is dealing with the assessment of risks involved in some decision, with the determination of the most efficient sampling inspection plans, with an analysis of the causes of variation in quality or with the interpretation of experimental results, whether obtained in the research laboratory or under production conditions.

That problems of this type are essentially statistical has for long been recognized by a number of individual workers in industry, but it is only within the last few years that any organised attempt has been made to bring these workers into contact with one another and to establish a link between them and the statisticians engaged in other fields. It is pleasant to record that the advance of this movement in England owes much to the stimulus resulting from the visit to London in May, 1932, of Dr. W. A. Shewhart of the Bell Telephone Laboratories. Dr. Shewhart not only gave a series of lectures in the University of London on the uses of statistical method in the control of eqality, but with the backing of such powerful organisations as the American Society of Mechanical Engineers, the American Standards Association and the American Society for Testing Materials was able to convince a number of men outside the academic field that there was something in this statistics business.

Much of the recent development in the theory of statistics has arisen in answer to a need for improved tools with which to handle problem in biological and agricultural research. The need here has been not only to interpret observational data, but to organise the planning of experiments so that they are likely to lead to conclusive results. With the knowledge that a steadily widening circle of scientific workers have accepted and welcomed the aid which he can give, the statistician might well feel confident that the value of his tools will be ultimately

recognized in the same way by those who are concerned with the scientific development of industrial efficiency. But faith without action is not enough. An absolutely essential step is the development of that contact between the statistician and the practical industrialist without which it is impossible for the former to learn how best to describe his wares in simple language, or to find real illustrations of their value which will convince the hard-headed business man to whom the neatness of a theoretical solution or the precision of an error determination mean nothing. As one who has been kept in touch with similar activities in the United States I am very glad to take the opportunity offered me by the Editor of the JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION to give an account of some of the ways in which this contact between the statistician and the industrialist has developed during the last few years in Great Britain.

THE BRITISH STANDARDS INSTITUTION

As the result of a conference, held during Dr. Shewhart's visit to London, to which the British Standards Institution invited representatives of manufacturing industries and others interested in the application of statistical methods, a committee was formed whose main objective was to report on the application and use of statistical methods in standardisation and specification of quality. The work of this committee has recently taken shape in the publication by the British Standards Institution of a small handbook[1] put together by the present writer in collaboration with the committee. Among other things its object has been,

(1) To provide a wide circle of readers with answers to the questions, Why is this technique needed? In what does it consist? What kind of assistance can it give?

(2) To give those who wish to apply the methods a reference book in which to find set out elementary methods, terminology, formulae, tables, etc., as well as a variety of practical illustrations.

(3) To show how the problem of securing conformity to a specification is essentially a statistical one in all cases where conclusions regarding a consignment have to be drawn from a sample selected therefrom; to discuss methods of dealing with this problem.

The committee were fully aware that this publication represented no final word on the subject; their purpose was to create interest, to

[1] *The Application of Statistical Methods to Industrial Standardisation and Quality Control.* B.S.I. No. 600–1935.

arouse discussion and comment, and so to make it easier to determine how far and in what ways statistical method can be most effectively introduced as an aid to the attainment of industrial efficiency.

RESEARCH STATIONS AND INSTITUTES

There are in this country a number of Research Stations attached to the Department of Scientific and Industrial Research as well as a wider circle of Research Institutes or Associations which form a link between that Department and the more important industries. In a number of these the use of statistical method has been established. The value of this technique in the planning and interpretation of routine tests and experimental research is quickly recognized by the young scientific worker whether he be physicist, chemist, engineer or biologist, and after some initial direction and guidance the statistical approach is acquired as a habit rather than assumed with an effort. Evidence that this approach has been accepted will be found, for example, in work published from the Building Research Station, the Forest Products Research Laboratories, the Fuel Research Station, the British Cotton Industry Research Association and the Woollen Industries Research Association.

On a recent visit to the Shirley Institute (Cotton Industry Research Association) near Manchester, on the occasion of the opening of a series of new Laboratories, I was particularly struck by the way in which the most modern statistical technique had become absorbed into the general procedure, whether this was connected with fundamental research or with routine investigations carried out on defective material sent in for report from the mills. There has been a trained statistician at Shirley for about 12 years and perhaps the highest tribute that one can pay to his work is not that he has built up any large statistical department under his control, but that he has so worked that the scientific staff has become statistically minded. This I think is the ideal situation; that except in cases where new problems arise requiring more expert advice, the common tools of statistical analysis should be handled freely and with confidence by those engaged in the planning and interpretation of technical research. This widespread adoption of the statistical outlook in the Cotton Industry Research Association is perhaps one of the most important pointers that we have to what may expected in the future.

THE UNIVERSITIES

The importance in the present connection of a University Statistical Department or Laboratory lies I think in the fact that there is here

some leisure to lay plans for increasing that contact between the statistician and the industrialist that is needed. I can speak only with experience of the situation as we see it in the Department of Statistics at University College, centrally placed in London, but it may perhaps be of interest if I mention some of the objectives that we keep in view.

(1) Whenever opportunity arises, to obtain suitable data which may be turned into illustrative material showing in simple terms some of the functions of statistical method.

(2) To direct the research of certain post-graduate students on to problems for which a solution is needed. If this involves the placing of a student for some time in a factory or its research department to watch processes or to collect data, the resulting contacts will have many advantages.

(3) To keep in touch with students who have permanent posts in industry.

(4) To welcome any opportunity of giving a training in statistical method to men having technical qualifications who may be sent from industry to work in the Department for a term or two, or even as part-time students.

(5) By a few well illustrated lectures to bring to the notice of students in other parts of the College, e.g., in the Department of Chemical Engineering, the existence and meaning of statistical tools.

(6) To provide evening lectures suitable for men already engaged in industry.

Throughout this work we are all learning, the teachers and the taught; I realise from my own personal experience how long a time it takes for the expert statistician to learn how unintelligible he may be, even when he thinks he is talking in simple language!

THE INDUSTRIAL AND AGRICULTURAL RESEARCH SECTION OF THE ROYAL STATISTICAL SOCIETY

This Section was formed in the autumn of 1933; its objectives have been set out fully in Part I of the Supplement to the Society's *Journal*.[2] By providing at its meetings a platform for discussion it plays an essential part in the movement I have described by bringing together not only the groups already referred to, but in addition the agricultural statistician. The methods of statistical analysis and the conception of planned experimentation whose origin is associated with the Rothamsted Agricultural Experimental Station and the name of R. A. Fisher,

[2] *Supplement to the Journal of the Royal Statistical Society* (Industrial and Agricultural Research Section) Vol. I, No. 1. 1934.

can and are being used by workers in the field of industry. As shown by its name, the Society's Research Section aims at increasing this contact between agriculture and industry.

The progress that the Section has made in the pursuit of its objectives may best be judged by reference to some of the papers that have been read before it.

The opening paper entitled "The application of statistical methods to production and research in industry" was a general survey given by Dr. R. H. Pickard, F.R.S., Director of the British Cotton Industry Research Association. Dr. Pickard spoke with the experience of one whose job is to collaborate with many people in industrial research work and who has felt, as he described it, the urgent necessity of applying statistical methods to a great deal of the work which is carried on by his colleagues.

The second paper on "Statistics in agricultural research" by Dr. John Wishart contained a clear description of much of the statistical technique developed in this country during the preceding 12 years. The methods of analysing the variation of heterogeneous material and the problems of sampling which it described are clearly of first rate importance in much industrial as well as agricultural research. The third paper on "Sampling problems in Industry" by the present writer discussed some of the wide variety of forms in which these problems arise.

There have since been two contributions from the Research Institutes of which I have spoken. Mr. B. H. Wilsdon, then at the Building Research Station, and now Director of the Woollen Industries Research Association, read a paper entitled "Discrimination by specification statistically considered and illustrated by the standard specification for Portland cement," and Mr. L. H. C. Tippett of the Cotton Industry Research Association gave a paper on "Some applications of statistical methods to the study of variation in quality in the production of cotton yarn."

After two years of activity there was a feeling that the discussions of the Section had not yet been of a kind which would catch the interest of the practical man who had little or no knowledge of statistical technique and terminology. The committee of the Section therefore decided to arrange for one or two discussions which would centre round an actual problem, presented by an industrialist, that had arisen in the course of the production with which he was concerned. The statisticians should then describe in as simple terms as possible the methods which they would use in interpreting the data already collected or in planning further investigation.

Following these lines a paper was presented last November by Messrs. Dudding & Jennet of the General Electric Co., Ltd., (England) dealing with a problem that had arisen in research into the quality of electric lamp filaments. An excellent discussion followed in which among other things, use was made of models and diagrams to illustrate the meaning of partial and multiple regression. A similar discussion is planned at the Section's Meeting in May when Dr. Hampton and Mr. Gould of Chance Bros. & Co., Ltd. (Glassmakers), will present a paper dealing with a problem with which they have been faced in the manufacture of spectacle glasses. This problem provides good material for illustration of the use of the methods of analysis of variance in picking out the factors in the production process which seem to be associated with greatest variation in quality.

LEGAL ASPECTS OF STATISTICAL ACTIVITIES OF TRADE ASSOCIATIONS WITH SPECIAL REFERENCE TO THE SUGAR INSTITUTE DECISION

By Charles J. Brand, *Executive Secretary and Treasurer*
The National Fertilizer Association

On March 30, 1936, the Supreme Court of the United States handed down its decision in the case of The Sugar Institute, Inc., the American Sugar Refining Company, and other appellants. The case was in the Supreme Court on appeal from the decision by Judge Julian W. Mack rendered March 7, 1934, in the District Court of the United States for the Southern District of New York. The suit was brought in March, 1931, in the lower court by the Department of Justice to dissolve The Sugar Institute, Inc., a trade association organized in 1927 by fifteen of the largest cane sugar refiners in the United States.

The organizers of the Institute had proceeded with the encouragement of the Department of Commerce and after conferences with the Department of Justice. A "Code of Ethics" under which the Institute was to operate was prepared by the refiners with the assistance of able counsel. In advance of operating under it the code was submitted to the Department of Justice for criticism and suggestions. It is understood that the Department gave such approval to the code as was warranted in the circumstances, but specifically reserved its freedom of action should it appear at any time that operations under the code had brought about or were likely to bring about unlawful restraint of trade.

The main feature of the "Code of Ethics" was the so-called "basic agreement" which provided that: "All discriminations between customers should be abolished. To that end sugar should be sold (by the refiners) only upon open prices and terms publicly announced." There was no agreement to fix prices, nor did the government allege any such agreement. As the Supreme Court pointed out, "the distinctive feature of the 'basic agreement' was not the advance announcement of prices, or a concert to maintain any particular basis price for any period, but a requirement of adherence, without deviation, to the prices and terms publicly announced."

Rarely has a case been prepared and tried more carefully. The testimony covered over 10,000 pages and included in addition over 900 exhibits. The opinion of Judge Mack covers 146 single-spaced mimeographed pages. It has never been printed and hence is not conveniently available to students of economics and statistics.

SUMMARY OF DECREE OF THE COURT BELOW

Judge Mack's decree filed on October 9, 1934, perpetually enjoined and restrained appellants from following 45 specific lines of action which were set forth in a like number of paragraphs describing each prohibited practice in appropriate detail.

Paragraph 1 prohibits effectuation of any plan to give the same terms, conditions, or freight applications to customers irrespective of the varying circumstances surrounding particular transactions or classes of transaction, and regardless of the varying situations of the customers with whom business was transacted.

Paragraph 2 prohibits selling only upon or adhering to prices, terms, conditions, or freight applications announced, reported, or relayed in advance of sale, or refraining from deviating therefrom.

Paragraphs 3, 4, and 5, quoted verbatim later herein, prohibited competitors from systematically and by agreement exchanging among themselves or reporting to a common agency, information as to current or future prices, terms, conditions, or freight applications, or lists or schedules thereof; from relaying such information by or through The Sugar Institute; and from giving prior notice of changes or contemplated changes, or relaying, reporting, or announcing changes in advance.

Paragraph 6 prohibits restraints on repricing.

Paragraph 7 deals with statistical information per se and prohibits the collection and/or dissemination even of such statistics as have hitherto been regarded as altogether lawful unless they are made appropriately available to the sugar buying and distributing trade. This paragraph is also quoted verbatim elsewhere in this article.

Paragraph 8 prohibits pressure upon third parties to accept and follow any program enjoined by the decree.

Paragraph 9 prohibits agreeing to meet, suppress, or restrain prices, terms, conditions, or freight applications of particular refiners or distributors.

Paragraphs 10 to 45 prohibit any agreement or concerted action with respect to each of the acts, trade practices, rules, policies, code provisions, or interpretations set forth in the separate paragraphs, such as:

1. Restraints on brokers and warehousemen by boycotting them for engaging in more than one distributive function.
2. Transportation restraints, particularly agreeing to sell only at delivered prices.
3. Concerted reduction in the number of consignment points.

4. Concerted prohibition on long-time contracts and quantity discounts.
5. Restraints on the sale of damaged sugar and so-called frozen stocks and upon resales of sugar.

DISCUSSION OF STATISTICAL ASPECTS

The legal status of so-called statistical information that a trade association may collect depends on whether the information collected really constitutes statistics.

The science of statistics deals with the collection, tabulation, and dissemination of facts. A fact is a thing done; an act performed or an incident transpiring, an event or circumstance. Court decisions also uphold the view that an event mental as well as physical is a fact.

The statistical activities of The Sugar Institute included the collection and dissemination of information on a wide variety of subjects pertinent to that industry. Among these were:

1. Production
2. Sales
3. Deliveries
4. Stocks on hand
5. Refining capacity
6. Stocks on consignment
7. Quantities moving at differential rates
8. Quantities moving over differential routes
9. Stocks in transit
10. Sales for export
11. New business
12. Stocks in consumers' warehouses

There is no indication that statistics covering prices in past transactions were included. All information concerning the business operations of the members and coöperating non-members was exceedingly intimate and detailed. The inaccessibility of Judge Mack's decision warrants quoting from it of the detailed statement as to statistics. Beginning at the middle of page 16 of the mimeographed opinion, Judge Mack says:

> I shall consider separately statistical matters relating to data (1) collected by the Institute (2) that as to which the Institute did not concern itself.
>
> 1. Most important among the Institute's statistical services exclusive of price and terms reports are the following: (all reports were compiled chiefly from data supplied by defendant refiners.)
>
> (a) Each week the Institute sent out an individual report to each refiner

showing the total weekly melt (i.e. production), deliveries and stock on hand of all members and the percentage thereof of the refiner so notified.

(b) The Institute reported weekly the melt and deliveries for the week of each member as well as his cumulative total melts and deliveries from the beginning of the year to the end of such week. Key letters were used to designate the several refiners; each refiner had a code of all designations.

(c) At the end of the contract period on each price move, a report or reports were sent out showing for each refiner the total undelivered and unspecified sugar on the contracts. Reports were also sent out showing by states the total amount of undelivered sugar for each refiner; here too, the key letters were used instead of names.

(d) Reports of capacities of the several refiners were circulated several times during the Institute period.

(e) An annual compilation of statistics collected by the Institute with analyses thereof was sent out.

The foregoing reports were furnished to each of the Institute members but to them alone.

(f) A quarterly statistical report was sent to Institute members and a few others, chiefly representatives of off shore refiners.

(g) A weekly report showing total deliveries in each state for such week by all refiners but not by each of them, was sent to the Institute members, Hershey and its sale representative.

(h) Each month a report showing total deliveries by states of all refiners for the month, together with a comparison with the same in each of the four years immediately preceding and the same data for the year to the end of such month, was sent to the same parties.

(i) A weekly report showing by states with some subdivisions thereof total sugars on consignment at consignment points for all refiners but not for each of them, was sent to the same parties.

(j) A similar report showing in-transit stocks was sent weekly to the same parties.

(k) Reports showing the amount of sugars moved into each state during the week by all the important differential routes[1] for refiners own account and separately, at customers' request, together with some analyses thereof were sent to the same parties; some such reports were also sent to L. W. & P. Armstrong, representatives of an off shore refiner.

(l) A monthly report showing the total cane and beet sugar deliveries separately by states was sent to Institute members, the Domestic Sugar Bureau and several representatives of off shore refiners.

With the foregoing description of The Sugar Institute and its operations, particularly its statistical activities, before us, we may proceed in the light of previous decisions to some evaluation of the effect of the decision in this case on the statistical activities of trade associations. It should perhaps be added that the refiners who were members of The Sugar Institute controlled between 70 and 80 per cent of the total sugar refined in the United States.

[1] That is, by water and combination water and rail routes, carrying lower freight rate than all rail service.

The decree of the lower court with respect to statistical activities, excluding the part that relates to the reporting of prices and changes of prices under the Institute's open price plan, enjoined, restrained, and prohibited, individually and collectively, members of the Institute, in connection with the sale, marketing, shipment, transportation, storage, distribution, or delivery of refined sugar, from engaging or attempting to engage, directly or indirectly, with one another or with any competitor, through any program, in:

> 7. Effectuating any system of gathering and/or disseminating statistical information regarding melt, sales, deliveries, stocks on hand, stocks on consignment, stocks in transit, volume of sugar moved by differential or other particular routes or types of routes, new business *or any other statistical information of a similar character*, wherever and to the extent that said information is not made or is not readily, fully and fairly available to the purchasing and distributing trade. (Italics added.)

The point of greatest significance in this prohibition is that when producers see fit to gather statistical information that is of legitimate interest to the purchasing and distributing trade it must be made readily, fully, and fairly available to such trade.

The Supreme Court upheld the prohibitions of paragraph 7 of Judge Mack's decision quoted above, except that it ordered to be stricken therefrom the italicised words, "or any other statistical information of a similar character," and the insertion of the word "and" before the words "new business." The general effect of paragraph 7 as changed, therefore is to permit the collection of a very wide range of statistical information, provided only that it is made available appropriately to the purchasing and distributing trade.

It is fairly inferable from the earlier cases that were carried to the Supreme Court, including the American Column and Lumber case, 1921 (257 U. S. 377), the American Linseed Oil Company case, 1923 (262 U. S. 371), the Maple Flooring Manufacturers Association case, June 1, 1925 (268 U. S. 563), and the Cement Manufacturers Protective Association case, June 1, 1925 (268 U. S. 588), the Supreme Court held and holds lawful the work of trade associations involved in *openly* and *fairly gathering* and *disseminating* information as to:

1. Cost of product.
2. Volume of production.
3. Actual price the product had brought in past transactions.
4. Stocks on hand.
5. Approximate cost of transportation from principal point of shipment to points of consumption.

6. Meeting and discussing such information and statistics without reaching any express or implied agreement or concerted action with respect to prices or production or restraining competition.

The Sugar Institute opinion is strictly in line with the views expressed by the court in 1921 in the Column and Lumber case in which the court especially objected to the fact that "published reports . . . go to sellers only." In the latter case the court adopted the method of stating a large number of activities or types of conduct that would be considered either questionable or undoubtedly illegal if engaged in by members of the unincorporated American Hardwood Manufacturers Association. Among these were the following:

1. Predictions in writing, sent to the manager of statistics and distributed by him, that high prices prevailing as shown by reports would continue to be maintained and enhanced.
2. Expositions of the theory that if each defendant had knowledge of existing prices, such knowledge would be sufficient to keep prices stable at normal levels even in the absence of agreement.
3. Sending out questionnaires and subsequently editing the answers and distributing a summary among members tending to show that the open pricing plan of the association produced a steady advance in prices.
4. Printing and distributing among members arguments against low prices based on reports of stocks on hand.
5. Urging members against increase of production by working night crews.

It is readily apparent, even to the layman, that the activities above described were not statistical. They were related to the collection and analysis not only of statistics but also of collateral material, and involved an unlawful use of the materials. Facts, views, prophecies, and other matter that did not constitute statistics were edited and then disseminated in such a manner as to involve an attempt to control conditions as they might arise in the future. Even data that were in fact statistical were used unlawfully.

In the American Linseed Oil Company case, involving the services of the Armstrong Bureau of Related Industries to the Linseed Oil Council, the Supreme Court dealt with the illegality of collecting and using data that were not statistical, but of such a nature as inevitably to "have only one purpose and effect, namely, to restrain competition among sellers." In this case real statistical data were gathered as well

as reports that were not statistical, e.g., the names of prospective buyers. The collection and dissemination of such information as did not constitute statistics when used in conjunction with the whole general plan were found to be illegal.

The decision of the Supreme Court in the Sugar Institute case has given us the most recent judicial interpretation as to what statistical data may be considered to be within the realm of absolute legality.

Earlier in this paper we quoted verbatim the description of the statistical work of the Institute as set forth in Judge Mack's opinion. Certain of the information gathered was furnished only to the members of the Institute. A part was furnished to members of the Institute and other competitors, both foreign and domestic. Another and minor part was given a certain degree of public dissemination. With the minor excision from paragraph 7 of the decree of the lower court given above, the Supreme Court upheld Judge Mack's desision as to statistical information. In its decision the court found that the data being collected and disseminated by the Institute were such that the purchasing and distributing trade had a legitimate interest in them and that they should be made available to the trade and the purchasing public.

OPEN PRICING

Although the price reporting activities of The Sugar Institute and its members also constitute a statistical activity in a sense, it is not my purpose to discuss them in detail in this paper.

The Supreme Court attached some importance to the facts that the appellants emphasized the desirability of a more nearly perfect competition and professed a policy of fostering such competition through prices, terms, and conditions publicly announced in advance of sale, and adhered to without deviation or discrimination.

Paragraphs numbered 3, 4, and 5 of Judge Mack's decree dated October 9, 1934 (equity 59–103), perpetually enjoined, restrained, and prohibited the Institute from:

> 3. Effectuating any system for or systematically reporting to or among one another or competitors or to a common agency, information as to current or future prices, terms, conditions, or freight applications or lists or schedules of the same;
>
> 4. Relaying by or through The Sugar Institute, Inc., or any other common agency, information as to current or future prices, terms, conditions or freight applications, or any list or schedule of the same;
>
> 5. Giving any prior notice of any change or contemplated change in prices, terms, conditions, or freight applications, or relaying, reporting, or announcing any such change in advance thereof;

The effect of these paragraphs of the decree of the lower court, had they been allowed to stand, would have been gravely to discourage lawful and fair anti-discrimination open pricing plans. Any open pricing plan becomes illegal and unfair if it involves any direct or collateral express or implied agreements as to what the prices openly announced shall be. Likewise, the utilization of coercion, intimidation, or reprisals in connection with prices constitutes an illegal use which causes the plan itself to become illegal.

The Supreme Court ordered modification of the decree of the lower court by striking out paragraphs 3, 4, and 5, with the statement that such reporting or relaying as permits voluntary price announcements by individual refiners to be circulated and relayed by the Institute, subject, of course, to the restrictions imposed by the decree, does not appear to involve any unreasonable restraint of competition, under conditions prevailing in the sugar industry.

In the Sugar Institute case, in holding that information may be gathered as to current or future prices, terms, conditions, or freight applications, or any list or schedule of the same, the court went farther than it has gone in the past. In accordance with trade usage, future prices, terms, and conditions of sale, and notices of contemplated changes, may be collected and disseminated by an association. The dissemination of this information to the purchasing and distributing trade, however, must be complete enough to afford them sufficient knowledge of commercial conditions and give them an equal advantage in the Association's collected data, insofar as they have a legitimate interest. In these circumstances, the activities of associations in compiling information are not limited to past transactions.

The court, in holding that information collected should be made readily, fully, and fairly available to the purchasing and distributing trade and to other persons having a legitimate interest therein, did not take the position that all statistical data collected must be disseminated. In fact the court held that there might be details of information received that might rightly be treated as having a confidential character, in which distributors and purchasers had no proper interest. It is obvious from the opinion that this confidential information must be rather limited, as for example, cost data, credit information, and business volume of individual enterprises.

CONCLUSION

The legal status of so-called statistical information which a trade association may collect is one which depends almost entirely upon whether or not the information so collected in reality constitutes statis-

tics. The legality of collecting and tabulating facts about things that have been done or occurrences that have taken place, as distinguished from information relating to things that have not happened and may never happen, is scarcely open to challenge.

As to the dissemination of statistical information, the opinion in the Sugar Institute case leaves no doubt that data collected by an institute or association composed of producers, if it hopes for legal protection, must be disseminated to the purchasing and distributing public insofar as they have a truly legitimate interest therein.

As to both collection and dissemination of information pertaining to any trade or industry, trade organizations are in no sense curbed by the Sherman anti-trust law so long as the information pertains to actual facts and is not gathered or disseminated to foster or promote any illegal combination in restraint of trade. Within the sphere thus defined, business organizations seem to be perfectly free to compile and disseminate any type of statistical data which may be of interest or benefit to the members of the industry, provided always that dissemination is not discriminatory. Associations must be careful not to restrict accessibility or release of information collected in any way that will deprive the purchasing public of benefits obtainable from information in which it has a legitimate interest.

THE POISSON DISTRIBUTION AND THE SUPREME COURT

By W. Allen Wallis
Columbia University

It is obviously impossible for a President-elect of the United States to measure the burden which the various tasks of the Presidency will impose upon him. Even those duties which are specifically prescribed by the Constitution are largely of a contingent character, and their extent cannot be accurately forecast. Certain such predictions of the amount of labor entailed in the various duties, even though they cannot be made exactly, undoubtedly would yield to statistical treatment aiming at estimation in terms of probabilities. But on the whole, such computations would be of virtually no practical utility and of very little interest even as purely statistical exercises.

There is however, one problem of estimating a burden which will be imposed by the Constitution upon a President-elect to which, conceivably, somewhat more than academic interest might sometimes attach. That problem relates to the duty stated in Article II, section 2, which requires that : "The President . . . shall nominate and by and with the advice and consent of the Senate shall appoint . . . judges of the Supreme Court . . . ."

In order to estimate how often a President will be called upon to perform this duty, it is necessary to estimate the number of vacancies on the Supreme Court which will occur during the four years of an administration. The most obvious approach to this problem is through the use of mortality tables. But, obvious as is the approach, the limitations making it totally unfeasible are even more apparent. If "the Supreme Court is the most successful health resort in the world," as has been alleged (without, at least to the present writer's knowledge, adequate statistical justification), then no life expectancy table short of one based on the vital statistics of Justices of the Supreme Court would prove adequate; and, of course, the available data of this sort are not sufficient to provide even an approach to the statistical stability requisite in a life expectancy table. Furthermore, the constitutional duty of making appointments to the Court arises not only from deaths but also from voluntary retirements. An additional overwhelming complication involved in this approach is the necessity of estimating the probable ages of new appointees if the calculations are to prophesy

for any length of time, even a year or two. In the face of all these difficulties it seems the better part of wisdom to abandon the search for a basis from which to calculate a priori probabilities, and to seek an empirical method of estimating the chance of vacancies on the Court.

Let us suppose that in January, 1933, for some reason, we were required to estimate the number of vacancies in the Supreme Court which would occur during 1933, during 1933 and 1934 combined, during 1933, 1934, and 1935 combined, etc.; and that we were also required to supply fiduciary limits for our estimates, in order to indicate the degree of confidence which might be placed in them.

If we go over the records of the Court and tabulate the number of vacancies filled each year, we find the distribution shown in Table I, in which the total interval from 1790 to 1932, inclusive, is divided into three periods according as the membership of the Court was six, seven, or nine members.[1]

While Table I indicates that the empirical probability that all members of the Court will survive a given year is 89/143 (.622) for the total period or 59/96 (.615) for the period 1837–1932, inclusive,

TABLE I

Number of years in which specified numbers of vacancies in the Supreme Court were filled, for the period 1790–1932, inclusive, and for the sub-periods 1790–1806, 1807–1836, and 1837–1932.

Number of Vacancies	Number of Years			
	1790–1806	1807–1836	1837–1932	Total
0	7	23	59	89
1	9	5	27	41
2	1	2	9	12
3	0	0	1	1
over 3	0	0	0	0
Total	17	30	96	143

it is desirable in making the estimates to find a function describing the series as a whole, in order to avail ourselves of all the observations and in order to have an indication of the reliability of the conclusions. For this purpose a Poisson distribution proves an excellent function on which to base our estimates. To fit a Poisson function to this set of observations is to assume implicitly that a particular vacancy is equally likely to occur in any of the 143 years (or, for the last subperiod, in any of the 96 years), and that the probability of its actually occuring in any specified year is small.[2] We are to regard the vacancies

[1] The data are, in the main, taken from the 1936 *World Almanac*, page 227; but since only those appointments which resulted from vacancies are tabulated, the *World Almanac* data were supplemented by reference to Charles Warren, *The Supreme Court in United States History*, Boston, 1932. Those appointments which resulted in increases in the size of the Court or in promotions within the Court are excluded on the grounds that our present concern is with estimating the amount of unavoidable work which the President-elect might anticipate on the basis of the existing setup of the Court.

[2] The two best discussions of the theoretical aspects of the Poisson function are contained in the original derivations of the function by the two writers who discovered it independently from quite distinct approaches. S. D. Poisson, *Recherches sur la Probabilité des Jugements en Matière Criminelle*

not as phenomena attached to the particular men of whose careers they mark the termination, but as phenomena attached to particular Justiceships, or, better, to the Court itself; hence the vacancies may be regarded as a homogeneous group of events whose distribution among the 143 years is random. There is, however, a sense in which the vacancies are not independent of each other; that is, the occurrence of a vacancy during any given year slightly reduces the probability of another vacancy occurring that same year, because of the likelihood that the average age of the Court will be lowered by a new appointment. This interrelation would be of some significance if the data included cases in which major portions of the Court were replaced in a single year, or what is related to this, if the data revealed cycles, with long periods in which no vacancies occurred followed by periods of frequent vacancies; actually, however, neither of these characteristics appears in the data.[3]

Only the data since 1837 are used in computing the estimates. Throughout this period the institutional status of the Court has been much as at present; and, with the negligible exception of the period 1863–1867 (when there were ten Justices) the size of the Court has been constant at nine members.

From Table I it is computed that during the 96 years from 1837 to

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et en Matière Civile, Paris, 1837, sections 73 (pages 189–190) and 81 (pages 205–207) derives the distribution to which his name has been attached as a special (limiting) case of the binomial distribution. W. C. Gosset, writing under the pseudonym "Student," "On the Error of Counting with a Haemocytometer," *Biometrika*, volume V (1906–1907), pages 351–360, deduces the Poisson function from considerations related to the frequency distribution of areas containing specified numbers of particles when Nm small particles are spread at random over a surface divided into a very large number, N, of equal areas.

L. von Bortkewitsch, *Das Gesetz der Kleinen Zahlen*, Leipsig, 1898, accepting the form of the distribution as given by Poisson, discusses its statistical characteristics and applies it to a number of cases—it was von Bortkewitsch who provided that classic example relating to Prussian cavalrymen killed by horses' kicks, which is almost universally used in textbooks to illustrate the Poisson distribution. Giorgio Mortara, "Sulla variazoni di frequenza alcuni fenomeni demografica rari," *Annali di Statistica*, series V, volume IV (1912), pages 5–61, develops a criterion of the applicability of a Poisson function and, in this paper and in one on "Le variazoni della mortalità da generazione a generazione in Svezia," *loc. cit.*, pages 63–81, applies the function to a number of sets of data. Lucy Whitaker, "On the Poisson Law of Small Numbers," *Biometrika*, volume X (1914–1915), pages 36–71, considers the theoretical aspects of the function and criticises at length the applications made by Gosset, von Bortkewitsch, and Mortara.

The best practical discussion of the use of the Poisson distribution is given by R. A. Fisher, *Statistical Methods for Research Workers*, section 15. H. E. Soper's paper, "Tables of Poisson's Exponential Binomial Limit," *Biometrika*, volume X (1914–1915), pages 25–35, gives for values of the mean between zero and fifteen, by steps of one-tenth unit, the theoretical relative frequencies corresponding to each value of the abscissa for which the expected relative frequency is as great as .000001—though as a matter of fact, if one has at hand a table of logarithms the theoretical frequencies may be easily and quickly computed from the fact that they are the successive terms in the expansion of

$$P = e^{-m}\sum_{x=0}^{\infty}\frac{m^x}{x!}.$$

[3] It might be expected that the appointment of an entire Court in 1789 would have introduced such cycles; one might anticipate a period of no vacancies, followed by several grouped together. Actually, however, the Supreme Court was of scant importance and transacted but little business during its first years, with the result that Justiceships were regarded sufficiently lightly to permit frequent resignations; in fact, the number of appointments per year is greater for the first of the three periods of Table I than for either of the others.

1932, inclusive, there occurred 48 vacancies in the Court, an average of .5000 per year. From the Poisson distribution determined by this value of m,[4] the probabilities of various numbers of vacancies occurring in a year are computed and converted into expected frequencies for comparison with the observed frequencies, as shown in Table II.

TABLE II

Probability of a given number of vacancies in the Supreme Court being filled within a year, as computed from the Poisson distribution for $m=.5000$; the expected number of years out of 96 in which a given number of vacancies will be filled; and the actual number of years during the period 1837–1932, inclusive, in which a given number of vacancies were filled.

Number of Vacancies	Probability	Expected Frequency	Observed Frequency
0	.6065	58.226	59
1	.3033	29.113	27
2	.0758	7.278	9
3	.0126	1.213	1
over 3	.0018	0.170	0
Total	1.0000	96.000	96

The value of χ^2 between the expected and observed frequencies shown in Table II, as computed by combining the "3" and "over 3" lines, is 0.6771; and since there are two degrees of freedom,[5] we find that if the true distribution were as described by the Poisson function for $m=.5000$ the probability of obtaining by random sampling a distribution differing from it as much as does this set of observations is $P=.713$. An additional check on the conformity of the observations to the Poisson distribution is provided by the fact that for a Poisson distribution the mean and the variance are equal. Computation from the distribution observed for the period 1837–1932, inclusive, provides as the estimate of the variance the value $s^2=.5053$. Again referring to the sampling distribution of sums of squares, we find that the probability that random sampling would yield an observed variance differing this much from a population value $\sigma^2=.5000$ is $P=.914$ (there being in this case 95 degrees of freedom for a χ^2 of 96). This value of P indicates that if m and s^2 were independent, random sampling would result in such close similarity in less than nine per cent of draws of this size; and it therefore creates a presumption that the distribution describing the data should have its mean and variance equal. The hypothesis of a Poisson distribution may, therefore, be regarded as in conformity with the data. It provides us with a basis for making the required estimates, which may be found readily by application of elementary probability principles.

[4] For the Poisson distribution the arithmetic mean is a *sufficient* statistic; i.e., it alone includes all of the information relevant to fitting the function which can be obtained from the observations. cf., R. A. Fisher, *Statistical Methods for Research Workers*, section 3.

[5] Two degrees of freedom are lost from the maximum of four because (1) the mean and (2) the total frequency of the theoretical distribution are determined by the observations whose conformity is to be tested.

As an illustration of the actual calculations from the distribution let us consider p, the probability that during a given year one or more vacancies will be filled, and q, the probability that during a given year no vacancies will be filled. We have already seen that $q=\epsilon^{-.5000}=.6065$, whence $p=.3935$. But in order to be on the safe side we may wish to use a probability so conservative that there is little likelihood of its being an overestimate; that is, we may wish to find a value of p, p', so low that if it were the true probability there would be only one chance in twenty that random sampling would yield an observed probability deviating from it as much as does the value p calculated from these observations. For this we take $m'=m-1.96s_m=.3578$. Taking $q'=\epsilon^{-.3578}=.6894$, we find that $p'=.3016$.

In Table III are given the probabilities that one or more vacancies will occur (and also the complementary probabilities that none will

TABLE III

Probability that one or more vacancies in the Supreme Court will occur during specified numbers of years (together with the complementary probabilities that no vacancies will occur during the same periods), computed both from the most likely value and from the lower fiduciary limit ($P=.05$) of the probability that within a given year at least one vacancy will occur (as determined from data covering the years 1837–1932, inclusive).

Number of Years	Probability			
	Best Estimate		Lower Fiduciary Limit	
	None	One or More	None	One or More
1	.61	.39	.70	.30
2	.37	.63	.49	.51
3	.22	.78	.34	.66
4	.14	.86	.24	.76
—	—	—	—	—
5.99	.05	.95	—	—
8.35	—	—	.05	.95

occur) for given periods of years from January, 1933, using both the most likely value of p and its lower fiduciary limit, p'.

In point of fact, of course, there have been no vacancies in the Supreme Court during the first three years of the term of office of the present President. At the time he took office, however, he might have estimated with a very high degree of confidence that there was less than one chance in three that he would not be called upon before March, 1936, to fulfill his oath of office in this respect. Indeed, the *best* estimate would have been that there were only two chances in nine that no vacancies in the Supreme Court would occur during the first three years of his administration.

ON THE COMPARABILITY OF DEATH RATES*

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A comparison of the "crude" death rates (deaths annually to 1000 inhabitants) in different countries, or in the same country at different periods, contributes to our knowledge of sanitary conditions only if the rates are not too much influenced by factors independent of those conditions. Certain population groups have naturally a high and other groups a low death rate, and the gross or average death rate of the population depends largely on the proportion which those groups make of the whole.

The most important difficulty from this source is the diverse age composition of different populations; this is surmounted as far as possible by standardization. But even that does not result in complete comparability because the age composition of the standard population adopted greatly influences the results. It is advisable, therefore, to use the "ideal" population of a life table, which yields figures of the population for which the life table speaks independent of its age distribution.

Statisticians seem to have dealt very little with the question of using a life table for obtaining comparable death rates. The reason is that life tables aim to show the expectation of life at various ages, and for that purpose death rates for the whole population are not needed. Another reason may be found in the fact that one of the principal aims of the "crude" death rate (the comparison with the birth rate) is objectless in the case of the life table population whose stationary character is founded on the perfect equality of birth and death rates.

What is the best method of calculating the death rate of an "ideal" population? I have recently dealt with this question[1] and will here explain in more detail the method there proposed.

Let us call A the sum of the 100 (or more) figures which we obtain by subtracting from a round number of new-born (as a rule, 100,000) the deaths to be expected in successive years, according to the death rates stated for each year of age. This sum A has several different meanings:

(a) the sum of the survivors to each year of age amongst a generation of 100,000 new-born;

* Readers are invited to send to the author (or the Editor) their comments on this note for possible publication in a later issue of the Journal. (Ed.)

[1] *Revue de l'Institut international de Statistique*, III (1935), pp. 129–141; esp. p. 136.

(b) the sum of the life-years to be expected at the beginning of the single years of age by the survivors;

(c) the contemporaneously living population, as it results from the "stationary" annual number of births and deaths of the past 100 (or more) years during which such population was born;

(d) the total number of years lived by that population during a year.

If we take the 100 figures and their sum in the sense of (a) or (b), they are relative to the time to come; taken in the sense of (c) they are based on the time past; in the sense of (d), on the present time. In the cases of (b) and (d) each of the 100 figures must be diminished in order to make allowance for the years not accomplished by a part of those who have reached them. In the case of (a), finally, the sum A cannot be used for any conclusion on account of the fact that those who have reached the second year are counted twice, those reaching the third year, three times, etc.

None of these different meanings of the sum A can directly be used for calculating an annual death rate on 1000 living.

A trustworthy annual death rate of the "ideal" population can be found only by taking the figures in the sense of (c) and by the indirect way of calculating (1) the whole duration of life of the contemporaneously living "ideal" population, (2) the average duration of life deduced from this sum, and (3) the average death rate which will be the reciprocal of that average duration of life.

As to (1), we cannot calculate the duration of life of a "real" population, because we know only the years already lived by each age class. For an "ideal" population we can add the years still to be lived (according to the expectation of life) to those already lived, and thus establish the probable duration of life for each of the 100 age classes composing the "ideal" population. This will be made up of those still living among the 10,000,000 born during the preceding 100 years. Taking as an example the following table, we obtain the whole duration of life for every age class[2] by multiplying the figures in column 2 by those in column 5 (see col. 6). The sum of the 100 figures is 385,000,000, and this divided by the number of the living, 5,500,000, yields the average duration of life, 70 years. From this we obtain the average death rate of 1000:70 = 14.29 yearly per 1000.

[2] Each of the 100 figures in col. 2 of Table I ought to be diminished on account of deaths occurring during the whole year of life. Only in order to simplify the calculation, let us assume that 100,000 are born simultaneously at the beginning of the year and that 10,000 die simultaneously just before reaching the second year, 2,000 just before reaching the third year, etc. Of course such a simplification would not be admissible if we sought to calculate the exact expectation of life; but that is not the case.

A mathematician has suggested the following formula:

$$\frac{1}{2}+\frac{2(1l_1+2l_2+3l_3+\cdots+\omega l)}{l_0+l_1+l_2+l_3+\cdots+l_\omega}$$

l_0 being the number of living-born, l_x the number of those who reach the age of x years, and ω the upper limit of age. When calculated according to this formula on the basis of a recent life table, the life-table death rate is often higher than the crude death rate. In Germany, for instance, the life-table death rate thus obtained for 1924–26, was 14.17 (see Table II), but the crude rate was only 11.91. The difference results from the decrease in the death rate during the life-time of the present generation. Owing to that decrease, the proportion of aged persons in the "real" population is lower than it would be if in their

TABLE I

Living persons		Years of life			Total years of probable duration of life (2)×(5)
Age (years)	Number	already survived	to be expected	Sum of (3) and (4)	
1	2	3	4	5	6
0	100,000	0	55	55	5,500,000
1	90,000	1	60	61	5,490,000
2	88,000	2	60	62	5,456,000
...	...	...	...	...	...
80	1,000	80	5	85	85,000
...	...	...	...	...	...
100	1	100	0	100	100
Total	5,500,000				385,000,000

youth the death rates had been as low as today. Obversely, the proportion of aged in the "ideal" is higher than in the "real" population, because it is calculated on the basis of the lower present death rates in all age classes. But the decrease of the death rates in old age could not be as great as at younger ages; and so obviously the greater *number* of old people produces a higher death rate in the "ideal" than in the "real" population. On the other hand, if there had been an increase in mortality during the life of the present generation, the crude death rate would be higher than that computed for the "ideal" population.

Even if the death rate of the "ideal" population is much higher than the crude rate, it does not mean that sanitary conditions are worse than the crude rate indicates, since obviously the higher death rate is due to the larger proportion of persons reaching a high age.

Table II gives the results obtained by this method for some countries (col. 5) compared with the crude rate (col. 6) and with the reciprocal of the expectation of life at birth (col. 7):

It is surprising to see that the differences between the crude death rates of the four countries (col. 6) are almost entirely due to differences in age composition, and dwindle or disappear as soon as the influence of this difference is eliminated (col. 5).

Owing to the failure to examine thoroughly the problem of calculating a death rate in the "ideal" population, misunderstandings occurred where it was tried. The most frequent misunderstanding consists in not considering the fact that as the expectation of life at birth holds only for children in the first year of age, or more exactly, at the moment of birth, the death rate resulting from the reciprocal of that expectation of life also represents only the expectation of death in the first year of

TABLE II

COMPARATIVE DEATH RATES BY THREE METHODS FOR UNITED STATES, GERMANY, ITALY, AND SCOTLAND

Country, year, and sex	Life table population			Death rate per 1,000		
	Total	Probable duration of life in years		of life table population in col. 4	of the real population	according to reciprocal of expectation of life at birth
		Total*	Average			
1	2	3	4	5	6	7
United States (whites) 1929–31						
Male	5,963,934	413,945,181	69.41	14.15	10.89	16.28
Female	6,318,033	454,077,063	71.87			
Germany 1924–26						
Male	5,650,271	394,812,002	69.87	14.17	11.91	17.42
Female	5,934,079	423,087,561	71.30			
Italy 1930–32	5,488,000	388,655,132	70.82	14.12	14.36	18.22
Scotland 1930–32						
Male	5,600,000	389,679,545	69.01	14.23	13.39	17.86
Female	5,950,000	428,538,511	71.87			

* Unlike Table I, the fact that deaths occur during the year of life is here taken into account.

life, but not the death rate of the whole population. By taking such a reciprocal as a death rate of the "ideal" population one may obtain for instance a rate as high as 18.18 per 1000, if the life expectation at birth is 55 years as in Table I. Obviously in calculating an average death rate it is necessary to take into account the fact that the expectation of life *increases* during the first years and diminishes by *less* than a year annually during the rest of life. Therefore the rate 18.18, and the figures calculated in this manner in column 7 of Table II, cannot be applied to the whole population because they do not take into consideration the figures composing the sum A.

The mistake in such a procedure is also illustrated by the following example: If in column 2 of Table I the number of survivors at the first and second year were 91,000 and 87,000 (instead of 90,000 and

88,000), the true death rate would be lower on account of 1000 dying now in their second year instead of their first; but the death rate as calculated from the expectation of life at birth would be unchanged, remaining 1000:55 = 18.18 per 1000 "per annum." So we find again that no conclusion concerning the death rate can be drawn from the *sum* of a life table, as such sum might be the same for two "ideal" populations in spite of differences in their sanitary conditions.

In view of this analysis it appears that death rates resulting from the "duration of life method" give the best measure of mortality, unaffected by the differences in age composition caused by oscillations in the preceding birth rates and death rates.

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NOTES

ORGANIZATION OF THE CENTRAL STATISTICAL BOARD

Organization of the Central Statistical Board under the terms of an Act approved July 25, 1935 (49 Stat. 498), was announced on March 11, 1936 by the Central Statistical Committee, consisting of the Secretary of Labor, Chairman, and the Secretaries of the Treasury, Agriculture, and Commerce. Stuart A. Rice, President of the American Statistical Association in 1933 and formerly Assistant Director of the Census, has been appointed by President Roosevelt as the Chairman of the new Central Statistical Board. The announcement reads as follows:

ANNOUNCEMENT OF THE ORGANIZATION OF THE CENTRAL STATISTICAL BOARD

Pursuant to authority vested in the Central Statistical Committee by Public No. 219, Seventy-Fourth Congress, approved July 25, 1935 (49 Stat. 498), and in accordance with the provisions of Executive Order No. 7287, dated February 10, 1936, we, the Secretary of the Treasury, the Secretary of Agriculture, the Secretary of Commerce, and the Secretary of Labor, severally, as the members of the said Central Statistical Committee, do declare by this proclamation that seven members have qualified for membership in the Central Statistical Board, created by the Act hereinbefore cited, and that said Central Statistical Board has been duly organized and that the organization was completed on March 11, 1936. The said date hereafter shall be known as the date of organization of the Central Statistical Board as provided for by the said Act and by the said Executive Order hereinbefore cited.

Dated at Washington, D. C., this eleventh day of March, A.D., 1936.

By direction and authority of the Central Statistical Committee,

FRANCES PERKINS
Secretary of Labor
Chairman, Central Statistical Committee

The membership of the statutory Central Statistical Board is set by the Act at not to exceed fourteen members, who are of two classes: designated members appointed in accordance with the provisions of Executive Order No. 7287, dated February 10, 1936; and elected members, who, under the terms of the same Executive Order are elected by the Chairman and the six designated members.

The part of the text of the Executive Order prescribing the method of selecting members is as follows:

> The thirteen members, other than the Chairman, of the Central Statistical Board (hereinafter referred to as the "Board") established by the said act shall be selected as follows:
>
> (a) Six members of the Board shall be *designated* members. One designated member shall be named by each of the following officers from among the officials or employees subject to his direction as such officer:
>
> The Secretary of the Treasury
> The Secretary of the Interior
> The Secretary of Agriculture

The Secretary of Commerce
The Secretary of Labor
The Chairman of the Board of Governors
of the Federal Reserve System

(b) Seven members of the Board shall be elected by the Chairman and the six designated members within two months after the date of the organization of the Board, which, for the purposes of this order, shall be the date that the Central Statistical Committee established by the said act shall declare that seven members have qualified for membership in the Board: *Provided, however*, that not less than four of such elected members shall be persons already in the service of the United States: *And provided further*, that at least one of such elected members shall be a person who is not, during his tenure of membership, a permanent paid employee of the Federal Government.

The members designated in accordance with the foregoing Executive Order are:

George C. Haas, Director of Research and Statistics for the Treasury
Oscar E. Kiessling, Chief Economist, Mineral Resources and Economics Division, Bureau of Mines, Interior
Mordecai Ezekiel, Economic Adviser to the Secretary of Agriculture
Ernest G. Draper, Assistant Secretary of Commerce
Isador Lubin, Commissioner of Labor Statistics
E. A. Goldenweiser, Director of Research and Statistics, Board of Governors of the Federal Reserve System.

Six of the seven elected members have been chosen by the Chairman and the six designated members. They are:

E. Dana Durand, Commissioner, U. S. Tariff Commission
Corrington Gill, Assistant Administrator, Works Progress Administration
W. H. S. Stevens, Assistant Chief Economist, Federal Trade Commission
Leonard D. White, Commissioner, U. S. Civil Service Commission
Ernest M. Fisher, Director, Division of Research and Statistics, Federal Housing Administration
Frederick F. Stephan, Secretary, American Statistical Association.

The seventh membership has not yet been filled.

The text of the Act under which the statutory Board will operate was printed in this JOURNAL, December, 1935 (Vol. XXX, No. 192) pages 714–716. The Board expects to continue to operate along the general lines of policy laid down in its *First Annual Report*, which covers the calendar year 1934. A *Second Annual Report* is now available.

PROGRESS OF WORK IN THE CENSUS BUREAU

COMPILATION OF ACCIDENT STATISTICS

The Bureau of the Census is planning to undertake the compilation of accident statistics. The Secretary of Commerce has advocated this; and the Accident Prevention Conference, which met in Washington last December upon the invitation of President Roosevelt, recommended that the Department of Commerce gather and analyze accident information. A bill mak-

ing provision for this work has been approved by the Bureau of the Budget and introduced in Congress.

No new machinery will have to be set up for compiling these statistics. The inquiry as planned will cover all deaths caused by accidents of any kind; and the existing organization of vital statistics in the United States, operating under the supervision of the Census Bureau, provides the necessary agencies for procuring the data. The certificates for deaths from accidents will be sorted out from the other certificates as they are received each month in the state registrar's office; and transcripts will be promptly forwarded to Washington, where the data can be immediately tabulated and the results published. The items on the death certificates include sex, age, race or color, and occupation of the decedent; also the place where the accident occurred distinguishing whether in industry, in the home, or in public place; also whether the injury was in any way related to the occupation of the decedent. It is obvious that these data already obtainable from standard death certificates without further inquiry will be of inestimable value as a basis for accident statistics. The compilation would be amply justified and worth while even if it never went beyond this. But for a study of the causes and the prevention of accidents, it is anticipated that additional information will be needed; and in order to secure this it is proposed to follow up a certain proportion of the accidents reported on the death certificates and obtain supplementary data on a special schedule or questionnaire. The appropriation asked for will make it possible for the Bureau to employ a special agent in each state to collect the supplementary data and otherwise aid in this undertaking. It is expected that he will work in close cooperation with the state registrar and as a rule will be located or have his headquarters in the registrar's office. The form and content of the special schedule will at the outset be more or less tentative or experimental, with the expectation that the kind and amount of detail which it is practicable and worth while to obtain will in the end be determined on the basis of actual experience.

NEW SERIES OF VITAL STATISTICS REPORTS

The month of January marked the inauguration by the Census Bureau of a new series of publications entitled *Vital Statistics—Special Reports*, designed to make available to the state registrars and public health officers, as well as to the general public, the most recent data released by the Division of Vital Statistics. Not only will original data be released in this manner but analytical studies and discussions of important problems in the field of vital statistics will be included, as well as news items of value to health, scientific, and public interests. These reports are rotaprinted; and in order to insure their widespread availability they will be mailed to a list of depository libraries which have agreed to preserve them, and will likewise be sent to the office of each registrar of vital statistics in states and large cities. The pages will be numbered consecutively through the year, at the end of which a table of contents and a cross index will be prepared with the idea that the series may be bound and preserved as a single volume.

Of the numbers thus far published, one (No. 4), for instance, gives the total number of deaths for the states and principal cities within the registration area for each year from 1920 to 1934, together with death rates based on estimated population; another (No. 6) gives the number of deaths in Continental United States from each cause by age, sex, and color or race for the year 1934. This corresponds to Table 6 of the annual mortality reports. Various other tabulations as they are completed will be included and published in this series of reports.

THE 1935 CENSUS OF AGRICULTURE

The first series of state bulletins, containing statistics of farm acreage and farm value by tenure, uses of land, and the principal crops and classes of live stock was completed in March, and the preparation of the second series was begun in February. The statistics now being tabulated cover farm population, movement from cities, farm dwellings, labor and days worked by farm operators for hire. The remaining items of the schedule not yet tabulated include fruits, vegetables, legumes, cows milked, forest products sold, and a number of minor items. A supplementary appropriation of $200,000 has been provided which will make it possible to complete these tabulations within the next six months.

J. A. H.

AN OCCUPATIONAL DICTIONARY

The need felt for many years both by private and governmental organizations interested in employment and vocational problems, for a comprehensive dictionary of occupational terms has stimulated the recent undertaking of the Occupational Research Program of the United States Employment Service of a compilation of brief job descriptions for occupations which show up in employment office work to be presented in dictionary form. Similar compilations have been prepared from time to time by various organizations covering certain industries. However, due to their restriction to specific fields, these compilations are frequently of limited general use.

It is planned that the dictionary being prepared by the Occupational Research Program will at completion contain definitions of all occupations of significance in the employment field, listed alphabetically, and related to their respective industry or industries by notations following their listings. The nearest approach to such a publication printed in English is the *Dictionary of Occupational Terms*, compiled by the Ministry of Labour of Great Britain based on the census of British occupations and published in 1927. Its use in the United States is quite limited for obvious reasons.

The terminology of the proposed dictionary will be based largely on that used in the occupational classification recently developed in connection with the statistical reporting system of the United States Employment Service. This system of classification is described in detail in the March issue of the JOURNAL. In conjunction with this terminology, which was obtained largely from industrial organizations, additional sources will be tapped, such

as the several occupational reports of the Census Bureau, other governmental agency reports, and additional information from industrial agencies.

The actual job descriptions will be obtained from and checked by representatives of industries, largely through facilities offered by the Employment Service offices. This information, although it will ultimately appear with its occupational titles listed alphabetically, will be obtained according to industry, and will offer the opportunity of presenting a section for the grouping of occupational titles according to industry should it seem advisable. Information for the occupations in several industries has already been obtained by the Occupational Research Program and incorporated in job specifications, also described in the March issue of the JOURNAL. The cotton textile, laundry, automobile manufacturing, and construction industries have already been covered, and job descriptions for hotel, restaurant, retail selling, lumber and millwork, and the foundry and machine shop industries are now in the process of preparation. Except for necessary changes to be made in order to conform with the desirable dictionary manner of presentation, information contained in the job specifications is immediately available as groundwork for the proposed dictionary.

Some of the details of presentation have not yet been decided upon, but the general plan will be as follows:

1. The main title (the most usual or important term) of each occupation will appear first, in its alphabetical order, followed by alternate titles for that occupation, distinguished by a difference in printing type.
2. The name of the industry in which the occupation is usually found will be given in abbreviated form, followed by a definition or summary of duties of the occupation within that industry.
3. In instances where the same title is used within one industry for several separate jobs (types of work) each definition will be listed.
4. For definitions in which the same title applies to different types of work in each of several industries, each definition will be listed (alphabetically according to industry), preceded by the name of the industry.
5. Alternate titles will appear in their proper alphabetical order, being referred to the main title for definition by a cross index notation.

The dictionary will not be regarded as authoritative or standard with respect to legal or official usage, but will serve as a publication containing occupational definitions compiled in the light of the best available information of ordinary usage. Care will be taken to achieve accuracy and to note variations, localizing the variations wherever possible.

NOMINATING COMMITTEE

In accordance with the provisions of the Constitution, President Joseph S. Davis has appointed a Nominating Committee to make nominations for officers to be elected at the next Annual Meeting. The members of the Committee are Robert E. Chaddock, Chairman, Leonard P. Ayres and Meredith B. Givens.

FREDERICK F. STEPHAN, *Secretary*

A CORRECTION

By a regrettable omission in editing, certain provisional figures in the Secretary's Report as it was prepared for the Annual Meeting were not revised before the report was printed. (See this JOURNAL, v. 31, March, 1936, p. 186.) The total number of members on December 1, 1935, was 1,741 instead of 1,742 and the increase over the previous report was 118 members instead of 119 members. In the membership statement for December 31, 1935, the figure 1,640 should be 1,638.

CHAPTER ACTIVITIES

THE ALBANY CHAPTER.—The Chapter had a dinner meeting March 20, 1936, at which eighteen members and guests were present. Mr. Frederick F. Stephan, Secretary of the American Statistical Association, gave a short résumé of the activities and plans of the Association. The activities of the Chapter Committee on State Statistics were discussed and the Committee authorized to enlarge its membership by inviting representatives of various state departments, irrespective of their membership either in the Association or Chapter. It was also suggested that the American Statistical Association be requested to appoint a committee on New York State statistics to work with or supplement the work of the Chapter Committee.

Professor E. H. Van Winkle of the Department of Business Administration at the Rensselaer Polytechnic Institute gave an excellent illustrated talk on "Common Pitfalls in Application of Statistical Formulae."

THE AUSTIN, TEXAS, CHAPTER.—After being inactive for a considerable period, the Chapter recently held two meetings. In February, a small group met informally to consider plans for regular meetings. On April 1, the first of a scheduled series of regular luncheon meetings was held at the Union Building of The University of Texas with 21 present. The principal speaker was Mr. J. C. Boyd, Jr., with the Gas Utilities Division of the Texas Railroad Commission. The title of his address was "A Statistical Study of Oil Production and Movement in Texas, 1935." Mr. Boyd explained the methods and presented the results of an intensive statistical analysis of oil production and movement in Texas made in connection with the promulgation and enforcement of proration orders by the Railroad Commission, which, under the law in Texas, is charged with this responsibility. The results of the study presented by Mr. Boyd and series of charts used in connection therewith are to be included in a forthcoming annual report of the Railroad Commission.

THE CHICAGO CHAPTER.—The Chicago Chapter held the third dinner meeting of its current year on January 22, 1936. Members and guests of the Chapter heard an address on the topic "An Economic Evaluation of the Oil Industry" by Dr. John W. Boatwright, Research Analyst for the Standard Oil Company of Indiana and President of the Chicago Chapter. Dr. Boatwright developed his subject by pointing out the principal determinants of

both the supply and demand of crude petroleum and its chief derivative products. Many analytical charts showing the interrelationships of supply, demand, stocks and prices were used in the presentation. The wide appeal of the subject and the speaker's approach thereto was indicated by an attendance of sixty-one even though the evening was one of the coldest on record in Chicago.

On February 19, Mr. John W. Scoville, Statistician of Chrysler Corporation, addressed a joint dinner meeting of the Chicago Chapter and the Investment Analysts Club on the subject "The Automobile Industry During the Recent Depression and Recovery." The development of the subject was illustrated by the use of many stereopticon slides. Of the total attendance of 91, over two-thirds were members of the Chapter.

Dr. Louis Wirth, Associate Professor of Sociology at the University of Chicago, addressed the Chapter on March 18 on the subject "The Role of the Urban Community in the National Economy."

THE CLEVELAND CHAPTER.—There have been five meetings of the Business Statistics Section during the present 1935–1936 season. At the September meeting, Professor F. W. Graff of John Carroll University was elected Chairman and Mr. J. T. Diebold of The Ohio Bell Telephone Company, Secretary. At the September meeting and at the meeting held in March of this year, the group made forecasts of business activity as reflected by the fluctuations of the Federal Reserve Board's index of industrial production. Mr. Cyril J. Bath, President of Cyril J. Bath and Company, delivered an address concerning England's recent economic recovery at the meeting in October.

Current real estate conditions in Cleveland were discussed by Mr. Raymond T. Cragin of Cragin-Morris and Company, realtors, at the December meeting. Mr. Cragin cited the drop in living unit vacancies in Cleveland from nine per cent in 1932 to four per cent at present as an indication that a housing shortage is imminent. The speaker stated that the city would experience a building labor shortage with construction only half as great as at the high point. He stated that a quarter of the building trades mechanics have died during the past decade and few, if any, have been trained. He thought that innovations and new materials will tend to reduce building costs. He said that the construction industry is not stimulated by decreased costs; that people do not build because of reduced costs but because they desire the buildings. The Annual Meeting of the Association in New York in December was discussed at the January meeting.

THE CONNECTICUT CHAPTER.—At a meeting of the Chapter on January 23, Professor Hudson Hastings of Yale University, discussed the various forms of inflation and the possibility of inflation in the United States in the near future. Professor Burton Camp, Wesleyan University, was elected President of the Chapter for 1936. Professor Hastings was elected Vice-President and Mr. Louis M. Nichols, General Electric Supply Corporation, Bridgeport, was elected Secretary of the Chapter. Mr. Paul Dorweiler, Aetna Life In-

surance Company, Hartford, was elected to the Executive Committee and Mr. L. G. Flick, International Business Machines Corporation, New Haven, Chairman and Secretary of the Business Section.

A joint dinner meeting of the Regular and Business Sections was held in New Haven on March 19. Professor Fred R. Fairchild, of Yale University presented "Proposed Federal Tax Legislation" and five specially invited guests participated in the discussion.

The discussion brought out the necessity of certain exemptions, like banks and insurance companies, the danger of such a tax as a punitive measure, the desirability of allowing the reinvestment of surplus earnings for legitimate purposes of the business without penalty, and the need of some cushion in prosperous times to provide for periods of subsequent depression.

THE PHILADELPHIA CHAPTER.—A dinner meeting of the Phildelphia Chapter was held on March 27th, with a program of "Business Recovery." Mr. Woodlief Thomas, Assistant Director of the Division of Research and Statistics, Board of Governors of the Federal Reserve System, discussed trends in production since 1929. After emphasizing the importance of breaking down general production curves into the component units, Mr. Thomas analyzed the important trends by industries and by durable and non-durable goods. While the value of agricultural production has shown moderate variations, non-durable goods industries showed wide fluctuations and durable goods extreme fluctuations. Recovery has been most marked in the non-durable goods lines. During the past six months the durable goods industries have advanced rapidly and there are indications of continued advance.

United States Commissioner of Labor Statistics, Isador Lubin, analyzed employment trends since 1929. Employment payrolls declined to their low point in 1932–33, the index of payrolls falling to 37. The drop was much more severe in the durable than in the non-durable industries. For durable goods employment fell to 35, in the non-durable goods to 65. In payrolls the durable goods fell to 27, the non-durable to 53. In estimating unemployment in 1935 Dr. Lubin pointed out that manufacturing employment was still two million below 1929, a million and a half in the durable goods and a half million in the non-durable. These two million might eventually be reabsorbed in the building and related industries. In the manufacturing, building and service industries a total of five and a half million would need to be reabsorbed in order to bring employment back to the 1929 level. In addition, some three million have reached working age since 1929 of whom approximately a million are in school who would not have been in school if the 1929 ratios had prevailed. These figures sum up to a total of 7,400,000 unemployed and to these must be added an estimated 2 million unemployment in 1929 bringing the total unemployment to 9,400,000. Dr. Lubin pointed out that while new industries will absorb a large part of these we must look to an increase of social services such as recreation, public health and education to aid in the process.

The third speaker, Dr. Eleanor Dulles of the Department of Industrial Research of the University of Pennsylvania, analyzed recent developments in prices. Dr. Dulles found very little evidence of inflation in the United States, in fact far less than would have been anticipated by students of the problem. She emphasized four factors making for inflation: the gold status of the dollar which has not been adequately discounted in prices; governmental activities such as the NRA and the AAA; the budget deficits and government spending; the general recovery in world trade. On the other hand there were certain moderating factors working against inflation. The people in this country are not afraid of inflation and have had little experience with it; the United States was the last important country to devalue its currency and consequently there was little speculation in the currency; there was very little temptation to ship credit out of the United States. There have also been changes in fundamental cost conditions, combined with large commodity stocks in the world markets and the early stages of recovery added substantially to these. Dr. Dulles emphasized that the consumer was perhaps unconcerned with inflation largely because food prices had remained low relative to prices of the last ten years. Certainly inflation had been very mild up to the present time.

THE PITTSBURGH CHAPTER.—The meeting scheduled to be held in December was deferred until January 9th at which time members of the Staff of the Bureau of Business Research, University of Pittsburgh, reviewed the reports presented at the 1935 Annual Meeting of the American Statistical Association.

Instead of holding the January meeting, the Pittsburgh Chapter accepted an invitation from the Pittsburgh Personnel Association to attend a dinner and discussion of the Social Security Act, which was held on Thursday, January 23, at 6:30 P. M. at the Duquesne Club. The speaker of the evening was Bryce M. Stewart, Industrial Relations Counselors, New York.

The February meeting, held February 27th, was addressed by Professor Montfort Jones, of the University of Pittsburgh. Subject: "What is Our Gold Reserve?" and "What Should be Done With It?" Professor Jones covered the first part of the subject thoroughly, but remarked that he would prefer leaving the second part to others.

The March meeting scheduled to be held Thursday, the 26th, had to be postponed because of the flood. It would have been interesting to have held the meeting on the appointed date, in order to have listened to a recital of the experiences during the flood of those who would have been able to attend. Pittsburgh is not alone in having suffered incalculable damage in the flood stricken area, and together with others is emerging rapidly from the catastrophe.

THE NEW YORK DISTRICT CHAPTER.—A dinner meeting of the New York District Chapter of the American Statistical Association was held on the evening of March 3, 1936, at the Roger Smith Restaurant, 40 East 41st Street. The general topic discussed was "Stock Market Problems of 1936,"

and the presiding officer was Mr. W. W. Cumberland, of Wellington and Company.

The first speaker, L. L. B. Angas, spoke on the subject of "Investment for Capital Appreciation." The speaker addressed himself particularly to pointing out the probable differences in the rate at which profits increased in different industries during the upward stage of the business cycle—Major Angas taking the position that we have now attained the half-way point in the upward cycle.

The speaker pointed out that industries begin to revive at different stages of recovery, consumers' goods industries reviving first, the capital goods industries lagging behind. The greatest fluctuations in earning power relate to the raw material industries, construction industries and the luxury goods industries. Another group of industries whose earnings increase rapidly in business recovery consists in those industries which cater to important deferred replacement demand, as in the case of railroad equipment, automobiles and paint. The geometric industries, or those new industries such as air conditioning, Diesel engines, aircraft, etc., also offer opportunities for great increase in profits, since their sales may be expected to grow in geometric ratio.

Mr. Frank Rizzo, of Clinton, Gilbert and Company, spoke on "The Outlook for Bank Stocks for 1936." Mr. Rizzo presented data showing the extent to which the assets of banks declined from 1929 to 1933, due to the decline of deposits and the large write-offs. These declines have been followed by a two-year period in which deposits have increased, and the net annual write-offs have decreased. The speaker dwelt on the favorable factors such as the economies in operations which have been introduced, the new fields of profitable business which have been developed, and the leverage factor due to the fact that the banks' activities are in effect financed by the depositors' money.

Mr. Charles J. Collins, of Investment Counsel, Inc., Detroit, Michigan, analyzed the present technical position of the stock market, and outlined the factors which might be expected to bring about more unfavorable psychology and to signal a decline in the general level of stock prices.

Mr. James F. Hughes, of C. D. Barney and Company, speaking on the subject of "Timing of Stock Market Control" presented charts covering the period 1881–1935, which indicated that during this period of five decades the major portion of bull markets occurred while business activity was below the computed normal, whereas the major bear markets occurred after business had increased beyond the normal figure. In the 50-year period from 1880 to 1929 a bear market occurred while business was below normal in only 25 out of 600 months.

Mr. William Martin, a governor of the New York Stock Exchange and a partner of A. G. Edwards and Sons, led the discussion. Mr. Martin pointed out that whereas we should never have stock market control, we should expect to have stock market regulation. He stressed the limitations inherent in stock market technique, pointing out that there is no formula for making money on the stock market.

THE SAN FRANCISCO CHAPTER.—Dr. H. R. Tolley, Director of the Giannini Foundation, addressed the Chapter on December 17, 1935. His subject was "Problems in a Continuing Agricultural Adjustment Program." At a meeting of the Chapter on January 23, 1936, Dr. George W. Dowrie, Professor of Finance at Stanford Graduate School of Business, discussed the Bank Act of 1935. The Chapter met again on March 10 and was addressed by Malcolm M. Davisson of the University of California, on the subject "The Outlook for California's Tax System."

Dr. Davisson pointed out that during the last twenty-five years two outstanding developments have occurred in the field of California taxation. The first of these has been a tendency toward a more diversified state tax system with less dependence upon property taxes. The extent of this diversification may be seen from the fact that during the biennium 1935–37 the State of California will collect over $300,000,000 in taxes. This sum will be raised from the following levies: retail sales tax, 39 per cent; gasoline tax and motor vehicle licenses, 29 per cent; bank and corporation franchise tax, 10 per cent; alcohol and beverage taxes and licenses, 7 per cent; personal income tax, 5 per cent; insurance tax, 4 per cent; and inheritance tax, 4 per cent.

The second development has been an expansion of state aid to counties and localities in order to secure more uniform and efficient performance of governmental services; the money coming from the broader tax sources open to state governments but closed to county and local governments. The expansion of state aid in California is demonstrated by the fact that during the current biennium of the expenditures of the State of California, amounting to over $350,000,000, about 56 per cent will be turned over to counties as subventions for public schools, social welfare and highways. California, in common with a large number of other states, is at the present time facing a budget deficit. It is variously estimated at from $25,000,000 to $100,000,000. The situation is further complicated by the fact that several of the existing tax sources are up for a vote of the people at the November election; for instance, the retail sales tax, the personal income tax, and the chain store tax which has not as yet gone into operation.

The retail sales tax, even with the exemption of food stuffs accorded by the last legislature, is open to serious question because of the fact that it lays a disproportionately large burden upon the man of small means. Despite this defect of principle it is difficult to see how at the present moment the State can sacrifice such a productive revenue source. Those interested in an improvement of the tax system will, however, look forward to the day when fiscal conditions will make possible the replacement of the present sales tax by a more highly selective luxury tax. The personal income tax, which is being attacked currently in the press as a poor man's tax, as well-nigh impossible to administer, as costly to collect, and as destructive of enterprise and initiative, should be considered as a permanent part of our tax system to balance the very large number of regressive levies under which the State of California has always labored. These attacks upon the income tax are for the

most part fallacious and grow out of the propaganda spread by vested interests. Experience of other states, notably Wisconsin, which has used a personal income tax since 1911, gives little support to the charges of those who attack the levy in California.

The remedy for these fiscal ills does not lie in the application of an undiscriminating economy axe to governmental costs, but rather in the establishment of suitable relationships between state, county, and local governments looking toward the elimination of duplication and waste, coupled with a reasonable revenue structure with due proportions maintained between income, property, business, and commodity taxes.

WASHINGTON STATISTICAL SOCIETY.—A meeting of the Technical Group of the Washington Statistical Society was held on Friday, February 27, 1936. Howard G. Brunsman, of the Federal Housing Administration, presented a paper "Determining the Size of Samples for Measuring Changes in Rent and Vacancy." The second paper was presented by W. Arthur Shelton, Bureau of Public Roads, on "Methods of Estimating Highway Traffic Volume."

STATISTICAL NEWS AND NOTES

BUREAU OF RESEARCH AND STATISTICS, SOCIAL SECURITY BOARD.—The economic and statistical research program of the Social Security Board is being developed in the Bureau of Research and Statistics, with Walton H. Hamilton as director and Ewan Clague as associate director. A wide variety of research functions will be performed in this Bureau when organization is finally effected. Immediate pressure exists for administrative statistics which require the maintenance of current reporting services in the fields of public assistance and unemployment compensation. These services involve cooperation with the various States.

In addition to this part of the statistical program, somewhat more comprehensive analytical statistics will be compiled from time to time. Although some of these data will be collected more or less periodically through the administrative organizations in the States, other quantitative data will be secured by special field studies based on local surveys and investigations. The field studies are to be undertaken as soon as a staff can be recruited.

This Bureau is organizing a field service to assist the States in establishing statistical divisions within those State departments dealing with public assistance and those responsible for the administration of unemployment compensation. The Federal field representatives will be located in the various regional offices established by the Board, and additional field personnel will be working from Washington, D.C. as headquarters.

Another type of research, requiring the use of statistics, but not wholly concerned with quantitative measurement, will be the administrative and appraisal studies. These studies must be made by the Board from time to time in the States, and are intended to analyze the standards of performance and to determine the degree of administrative efficiency in the State

organizations. The Bureau of Research and Statistics, through its representatives, will participate in these administrative studies in cooperation with other bureaus of the Board.

A special division of research cooperation is being established so that the vast amount of research which is now going on and which is soon to be under way in the field of social security may be more effectively integrated. The Bureau plans to have its representatives establish and maintain close relations with other groups engaged in research—other Federal agencies, State and local public agencies, private research foundations, universities, and other research organizations interested in economic and statistical studies. If such integration can be attained, the Social Security Board may be able not only to foster research in problems relating to social security, but also to obtain the maximum advantage of such work as is done outside the Social Security Board.

Finally, a long-range research program is being developed to meet the specifications laid down by the Social Security Act. The Social Security Board, under the provisions of the Act, has "the duty of studying and making recommendations as to the most effective methods of providing economic security through social insurance, and as to legislation and matters of administrative policy concerning old-age pensions, unemployment compensation, accident compensation, and related subjects." In carrying out this duty imposed upon it by Congress, the Social Security Board will undoubtedly rely to a considerable extent upon the Bureau of Research and Statistics. Sustained studies of this type are to be made by the members of several divisions now being established. Such studies, it is hoped, may in the not too distant future have some influence upon the kind of social security legislation which will be operative in this country.

The Social Security Board is under the merit system, and, therefore, positions in the Bureau of Research and Statistics are filled through the Civil Service. It is now planned to hold a civil service examination for such positions sometime during the coming summer. Positions in a wide variety of grades, ranging from P-1 at $2,000 to P-6 at $5,600 minimum, will be open to the successful candidates in these coming examinations. Statisticans and economists with all types of background, but especially those with some previous work and experience in studies relating to social security, should consider making applications for these examinations.

DIVISION OF RESEARCH, STATISTICS, AND RECORDS, FEDERAL EMERGENCY RELIEF ADMINISTRATION.—Reports concerning relief statistics are continuing to be received from the States in simplified form. It is anticipated that these reports will continue on this simplified basis, thus maintaining general comparability with the relief statistics of the last three years. Although in certain States changes in the organization and administration of relief activities have occurred, considerable effort is being expended in attempting to obtain data from each State covering the number of families, cases and persons receiving general public assistance and the distribution of the cost of such relief by sources of funds. It has been necessary to change the de-

finition concerning the kind of relief to be reported from emergency relief from public funds to general relief from public funds thus including local poor relief but excluding categorical relief.

The Division of Research, Statistics, and Records and the Social Research Division have collaboarated in preparing a schedule for the collection of relief statistics in Rural and Town Sample Areas. These areas include 322 counties selected as representative of all counties having no cities of 25,000 or more population in 34 States together with 23 towns in Massachusetts representative of all Massachusetts towns having less than 5,000 population. Data are being collected on an agency basis for all outdoor relief activities, both general and categorical, extended from public or private funds in the sample areas in January 1936. The result of this exploratory study will determine the desirability of establishing on a sample basis, a new current reporting series in the field of social statistics.

Continued efforts are being made to present statistical and descriptive summaries of the programs and activities of the Federal Emergency Relief Administration. These summaries are being published in the *Monthly Report* of the Federal Emergency Relief Administration.

DIVISION OF SOCIAL RESEARCH, WORKS PROGRESS ADMINISTRATION.—*Census of Occupations of Workers on Relief.* The Division of Social Research is assembling comprehensive data upon the occupational characteristics of workers included in the relief population. In May, 1934, an intensive study was made in 79 cities covering the personal and occupational characteristics of a cross section of the urban relief load at that time. Information was provided which has been used to indicate the degree of employability of the relief population. A final report upon this study is to be issued this summer.

In connection with the preparations for and the administration of the Works Program, two complete censuses were taken of the workers on relief, the first in March, 1935, and the second in January, 1936. These complete census counts, each covering over six million workers, have provided information upon the usual occupations of this army of workers. In March, 1935, the survey included information upon age, educational attainment, residence, color and sex, in addition to the data upon usual occupation and industry of usual employment.

The January, 1936, survey included all workers who had been certified as eligible for employment on the Works Program. This census was essentially a recount of the workers covered in March, 1935, showing the shifts that have occurred over the ten-month period since the earlier count. These series of studies taken together will provide a wealth of basic material upon the incidence of unemployment which is not available elsewhere.

Studies of Relief and Works Program Experience in Thirteen Selected Cities. An extension of the studies of relief accessions and separations in 13 cities throughout the year 1935 includes an examination of a cross section of the relief load as of May and October, 1935, together with a month to month summary of the status of a selected group of cases on relief between May and October, in order to describe their relief and work experience following

the withdrawal of federal relief funds. This current survey will show the successive stages whereby relief families have continued their dependent condition or have raised themselves above a relief status. Most previous studies have been limited to the relief period itself, without re-examining cases after they have left the relief rolls. A somewhat similar examination of the post-relief status of rural families is being conducted in seven states.

Rural Relief Reporting. A study has been undertaken in 345 counties of the public and private assistance provided in rural and town areas. It is planned to collect statistics concerning cases aided and obligations incurred for material relief by all welfare agencies, public and private, in the selected counties for selected months. It has been contemplated in the selection of sample counties that these reports may provide a basis for continuing monthly records of rural relief analogous to the urban records compiled by the U. S. Children's Bureau. Final plans for such current reports have not been completed, however, although the official relief reports of the F.E.R.A. will provide basic monthly data for the most important part of the rural relief load. Further plans are being projected in coöperation with the Social Security Board and other agencies operating through a committee of the Central Statistical Board.

A number of other studies for which final reports are contemplated to be issued this year are as follows: Inter-city Comparisons of Living Costs for 59 Cities; An Analysis of the Nature and Scope of the Relief Problem; A Study of Migrant Families; a summary report upon the Changes in the Relief Populations of 13 Selected Cities; a study of Rural Families in Combined Farming-Industrial Employment; Farm Rehabilitation Problems in the Drought Area of the West; a study of the Plantation Tenant Situation in the South. Present plans call for the assembling and summarizing of important findings of previous studies in the publication of a number of monograph reports on various topics related to relief.

Among the publications recently issued by the Division of Social Research are included the following: Research Monograph II, *Comparative Study of Rural Relief and Non-Relief Households;* the survey was made during the winter of 1933–34 in 47 counties in the major agricultural areas of the United States. Research Monograph III, *The Transient Unemployed;* this report presents an exhaustive analysis of the transient relief population, their movements, their reasons for migration, and the problems involved in the reabsorption of this group into private industrial employment. Pictograph Booklet, *Youth on Relief,* presenting some of the more basic facts relating to the characteristics of youth (persons aged 16–24) in the United States, with special reference to youth on relief; the booklet was made up primarily for the National Youth Administration and contains thirteen charts. A Research Bulletin concerning *Intensity of Relief, July 1933–June 1935* was issued as Series I, No. 18. Two Research Bulletins on *Reasons for Failure to Accept Assignments to Works Program Employment* have been released, one covering urban and rural communities of Ohio, and the other covering 13 cities.

BUREAU OF LABOR STATISTICS, U. S. DEPARTMENT OF LABOR.—Adjustment of the indexes of factory employment to conform with the 1933 Census of Manufactures totals is practically completed. This adjustment eliminates any bias shown in the Bureau's reporting sample between 1931 and 1933 and therefore presents a more accurate picture of the levels of employment.

In connection with the work of the Pennsylvania Railroad in electrifying its lines through a Public Works Administration loan, the Bureau of Labor Statistics has secured monthly reports on employment, pay rolls, and man-hours worked on the job, classified by States, detailed accounts as to the value of material purchased, and the man-hours of employment created in the manufacture of such materials.

Reports on the amount of production in State prisons, the value of product, and detailed information on the health and well-being of prisoners have been completed for Maryland, West Virginia, and Kentucky. It is anticipated that this study, undertaken at the request of the Prison Industries Reorganization Administration, will ultimately cover all States.

Field work on a survey of wages and hours and working conditions in the cigar industry was scheduled for completion by May 1. The study covers 15,000 employees representing 25 per cent of the total employees in the industry. Data for the two pay-roll periods are included, that is, March, 1935, when the N.R.A. code was in effect and March, 1936, in the post-code period.

The annual survey of union wage rates is scheduled to begin on May 15, 1936. This report will bring up to date the series of statistics on wages of workers bargaining collectively with their employers on working conditions. The investigation, in accordance with the usual custom, will cover important industries and occupations in various geographic locations throughout the country. While the results for 1934 and 1935 have not been published in their entirety, the computations are completed and will be supplied on request.

The Bureau of Labor Statistics is engaged upon a program of revision and expansion that covers every phase of its wholesale price reporting service. Among the more important revisions undertaken are a detailed description of the items included in the price index, an enlargement of the commodity and industry coverage, methods dealing with geographic variations in the price structure, the type and method of weighting and index computation, a more complete classification of the commodities and industries, and means of increasing the effectiveness of the published data. The Bureau has completed or is now engaged in revision of the following groups of items: Farm machinery, soap and soap products, paper and paper products, chemicals and drugs, mixed fertilizers and fertilizer materials, lumber and mill products, building board, cement, brick and tile, and other building materials.

U. S. TARIFF COMMISSION.—*Comparative Statistics of Imports into the United States for Consumption, by Countries, from 1931 to 1935:* Under the supervision of the United States Tariff Commission a staff of about 160 employees, taken

mainly from relief rolls, is engaged on a W.P.A. project at Richmond, Virginia, in tabulating and preparing for publication detailed statistics of imports for consumption by countries for the years 1931 to 1935 inclusive.

Beginning in 1934 detailed statistics of imports for consumption, which include imports for immediate consumption plus withdrawals from warehouse, are being reported in *Foreign Commerce and Navigation*, by countries. Prior to that time the only statistics showing imports by countries for commodities were those of general imports which include all entries into the United States whether for immediate consumption or for entry into bonded warehouses. Aside from the above indicated difference between general imports and imports for consumption the more detailed commodity classification of the latter affords a much more comprehensive basis for making year to year comparisons. The needs of the Trade Agreements Program for detailed import statistics broken down by countries emphasized this deficiency and prompted the Tariff Commission to suggest the present project.

The statistics for 1934 have already been published by the Department of Commerce in *Foreign Commerce and Navigation* and are to be included in the reports of the Commission with such minor adjustments as may be necessary to make them comparable with the figures being compiled for earlier years. With the permission of the Department of Commerce the Commission is also enabled to compile and include the preliminary figures for the statistics for 1935 in its reports in advance of their publication in *Foreign Commerce and Navigation*. The basic data from which the statistics for 1931, 1932, 1933, and 1935 are being compiled are the code sheets supplied by the Division of Customs Statistics of the Department of Commerce. While this coded material has been used constantly by the United States Tariff Commission in securing the imports of commodities from certain countries there has never been such a large or complete compilation as is now being undertaken.

Separate reports will be published for each of the 11 commodity groups shown in *Foreign Commerce and Navigation* and the classification of individual commodities will be that followed in 1931 so far as practicable. The first report to be made available, probably in April, will be for Group 7, Machinery and Vehicles, and the others will follow as rapidly as possible. It is planned to have the entire work completed by August.

Requests for this material specifying the particular group or groups wanted may be made to the Secretary of the United States Tariff Commission, Washington, D. C. Such requests will not be acknowledged but will be filled as the reports are issued.

BUREAU OF FOREIGN AND DOMESTIC COMMERCE.—*The balance of international payments of the United States in 1935* will be available within a few weeks. This bulletin has been prepared by the Finance Division and continues the annual series initiated in 1922. Two special studies relating to the balance of payments are in preparation. One of these will be a study of foreign investments in the United States and will present the results of the

first comprehensive survey in this virtually unexplored field. The other study will be a detailed analysis of international insurance transactions affecting the balance of payments of the United States. The latter covers the period 1919–1935.

The *Statistical Abstract of the United States, 1935* and the *Foreign Commerce Yearbook, 1935* were released in February. The *World Economic Review, 1935* will be available soon.

After a year of experimenting, the Division of Foreign Trade Statistics has perfected a statement to show from month to month the various changes and trends of our foreign trade. This statement, called *Trend of United States Foreign Trade*, is available on a subscription basis at one dollar a year. The first of these monthly statements was published in April. These statements show the total trade for the monthly period, trade by economic classes, by leading commodities, and by geographic regions. The balance of trade, quantity, and unit value (price) indexes are also shown. All data are presented on a cumulative basis, and will be compared with corresponding data for the high year 1929, the low year 1932, and the year immediately preceding the one covered by the statement.

The Division of Economic Research has been pushing ahead with its study of "Long-Term Debts." Although no date has as yet been set for the completion of the study, statisticians will be interested to know that the study will deal with the following principal topics: (1) changes in the volume of long-term indebtedness by principal classes of borrowers from 1913 to date, (2) changes in the annual interest charges on the several classes of long-term debt during this period, (3) the volume of obligations in default at different dates, (4) the comparative ages of the several classes of debt outstanding in 1933, and (5) the ownership of the evidences of debt as of a recent date.

Members of the Association who are interested in current statistics on retail sales trends should not overlook the article on the development of the Bureau's retail sales indexes which appeared in the February *Survey of Current Business*. To the coverage of independent store sales by kind of business by states announced in that article, it is hoped to add sales reports from Arkansas, Iowa, Kansas, Missouri and Nebraska during the summer months. It is anticipated that in a few weeks the first of a series of regular monthly releases will be made on manufacturers' and wholesalers' sales and collections by kinds of business. This project is being worked out by the Bureau in cooperation with the National Association of Credit Men. Tabulations on the annual "Retail Credit Survey" are progressing and the study should be available shortly.

Market Research Sources, a guide to information on domestic marketing, was released April 10th. *Some Sources of Current Trade Statistics* is still in preparation. Among forthcoming publications, mention should be made of the *Consumer Market Data Handbook*, *Confectionery Distribution in the United States, 1935*, *Markets for Plumbing and Heating Facilities in Residences*, additional reports on the *Consumer Use of Selected Goods and Services, by Income Classes*, and *World Chemical Developments, 1935*.

FEDERAL TRADE COMMISSION.—In January, the Commission sent to Congress its second report on the Milk Industry based upon data collected in the Philadelphia and Connecticut sheds. Printed copies became available for distribution in March (House Document No. 387, 74th Congress, 2nd Session). The report contains, among other things, an analysis of the base surplus plan as it has been employed in these two milk sheds. This shows the methods that have been employed to permit the sale at Class I prices of milk for which the producers paid the farmer at Class II and Class III prices.

Of more immediate interest to statisticians are the analyses of the sales, expenses, and profits of the milk distributors in these sheds by class of product, i.e., fluid milk, fluid cream, skim milk, butter, cheese, ice cream mix, etc., and subdivided for sales at wholesale and sales at retail. The report also contains an interesting study of the effects of different methods of cost allocations on unit delivery costs of various products, indicating that the cost of delivering milk, cream, or any other dairy product can be made either higher or lower at any particular time by merely changing the formula for allocating the delivery costs between products.

Early in March, the Commission released certain preliminary figures covering sales, costs, profits, investment, and rates of return for cotton textile companies in the first half of 1935. Earlier in the year, the Commission released similar figures for cotton spinning companies showing results separately for combed yarn, carded yarn, and combination combed and carded yarn companies in 1933 and 1934.

The Commission's Agricultural Income Inquiry is well advanced. The comprehensive character of the data called for by the Congressional Resolution, however, has rendered it difficult to obtain prompt returns. As a result, a great deal of supplementary field work is necessary in an effort to expedite the reports.

At this writing, hearings are being held on a resolution looking to the extension of the Inquiry to fresh fruits and vegetables. Hearings are also under way on a resolution directing the Commission to investigate the farm machinery industry and trade.

INTERSTATE COMMERCE COMMISSION.—Among the new statistical items available monthly in the reports of steam railways to the Interestate Commerce Commission are the following: (1) average price at point of purchase of coal, fuel oil, electric current, gasoline, and Diesel fuel; (2) quantities of fuel and power purchased, divided as under (1); (3) number of units of fuel and power equivalent to one net ton of coal; and (4) quantity of fuel and power consumed per yard-switching locomotive hour for steam, electric, gasoline, and Diesel locomotives separately.

BOARD OF GOVERNORS OF THE FEDERAL RESERVE SYSTEM.—The text of the Annual Report of the Board of Governors of the Federal Reserve System for 1935 was released in April. The report contains an analysis of the business and credit situation in 1935 with special reference to the development of

excess reserves in that and prior years. It gives the results of a special investigation into the reserve position of individual member banks, made for the purpose of ascertaining the probable effect of an increase in reserve requirements upon the banks. A full record of policy actions taken by the Board since August 23, 1935, is published in the appendix to the report in accordance with the provision of the Banking Act of 1935 requiring the publication of such a record. The full edition of the annual report giving detailed statistics for 1935 and in some cases for earlier years has been sent to the printer and should be available before or shortly after the issuance of this JOURNAL.

FEDERAL HOME LOAN BANK BOARD.—The principal project of statistical importance since last fall in the Division of Research and Statistics is the development of a foreclosure index for metropolitan cities in the United States. This index or a chart based upon it is to be published in the April *Home Loan Review*. The March issue of the *Review* contains some new data with regard to the investments of insurance companies. A program of monthly reporting of new loans of building and loan associations is expected to be put in operation this summer. An index of the activity of building and loan associations with some breakdown as to loans for new construction and refinancing should be of considerable value from the standpoint of an early indication of revival in the home loan mortgage market.

FARM CREDIT ADMINISTRATION.—The Economics Subdivision of the Division of Finance and Research has recently begun a study of livestock financing. The chief objective of study is to determine the total debt carrying capacity of a ranch in relation to its capacity to produce income. The two principal sources which will be used in seeking an answer to this question are, first, the lending experience of the Federal land banks, the production credit associations and other agencies discounting through the Federal intermediate credit banks; second, information on a number of ranches, showing total long and short term indebtedness over a period of years, which will be studied as a basis for determining the relationship between the annual income of the ranches and their inventory values. Mr. Frank ImMasche is now on a trip through the West to gather data for this study. He will visit ranches in the Omaha, Wichita, Houston, Berkeley, and Spokane districts and will also study the records of the Federal intermediate credit banks and agricultural experiment stations throughout the area.

A study of the problems involved in financing spring wheat farmers has been started recently by the Economics Subdivision of the Division of Finance and Research. Dr. W. L. Cavert, Statistician of the Farm Credit Administration of St. Paul, has been granted a leave of absence for three months in order to take charge of the project for the Washington office. The territory included is located for the most part in the States of North Dakota, South Dakota, and Montana. The immediate objective of the study is a method of scheduling payments on extensions adjusted to annual variations in the

income of the wheat farmer. Dr. Cavert will spend the major portion of his time in the field obtaining data on income variations and in testing out the practicability of various suggested plans.

BUREAU OF AGRICULTURAL ECONOMICS.—A general survey of the collection, compilation, and publication of statistical series of the Bureau, including detailed study of the several kinds of problems, has been undertaken by a committee consisting of F. V. Waugh, chairman, O. C. Stine, Joseph A. Becker, F. L. Thomsen, Charles F. Sarle, and Mordecai Ezekiel.

Dr. F. L. Thomsen, for several years a professor of agricultural economics at the University of Missouri, has been appointed a principal agricultural economist in the Bureau of Agricultural Economics, and will share the administrative work of the Division of Statistical and Historical Research with Dr. Stine, in charge.

The Division of Agricultural Finance is directing a study of official county data on farm mortgages filed and released, farm land transfers and values, and farm real estate taxes, sponsored by the Works Progress Administration. Work will be done in all of the states, in about 1,000 counties, and as far as possible will cover the period since 1917.

The influence of weather and other factors on yields per acre of wheat and corn is a subject of statistical analysis recently begun under the direction of Dr. Charles F. Sarle. The project includes a historical survey and critical analysis of previous work done toward making long-range weather forecasts, and a statistical study of the planetary and lunar positions with relation to terrestrial weather, by Larry F. Page, associate meteorologist.

NATIONAL RESOURCES COMMITTEE.—The Science Advisory Committee to the National Resources Committee established a Committee on Population Problems on November 12, 1935. The Committee is composed as follows: Drs. E. B. Wilson, William F. Ogburn, Charles H. Judd, Warren S. Thompson, David Edsall, and L. C. Gray.

This Committee has been requested to present a report on national population problems during the summer or early fall of the present year. Dr. Frank Lorimer, Secretary of the Population Association of America, has been appointed Technical Secretary. Other technical members of the staff are: Dr. Harold F. Dorn, Professor Rupert B. Vance, Dr. Robert M. Woodbury, Professor Newton Edwards (representing the American Council on Education), Dr. Mildred Hartsough, and Miss Louise Blodget.

U. S. PUBLIC HEALTH SERVICE.—Field work of the health inventory (this JOURNAL, December, 1935, p. 746) is nearing completion and the coding of schedules is under way at the field headquarters in Detroit. Cooperating with the Census of Business, the Service has sent schedules to all hospitals in the U.S.A. and will tabulate the data. A study of public health facilities has been undertaken in the 91 cities included in the health survey and in the counties in which these cities are located.

CHILDREN'S BUREAU, U. S. DEPARTMENT OF LABOR.—With the cooperation of the Subcommittee on Stillbirths of the American Public Health Association and of individual hospitals, the Children's Bureau has under way a study of stillbirth mortality in hospitals. The purpose of the study is to obtain statistical information regarding fetal and maternal conditions associated with fetal mortality, to make possible the development of a classification of causes (fetal and maternal) of stillbirths, and to further the development of a special certificate for the registration of stillbirths which may serve as a base for comparable statistics for the various States.

The study is an outgrowth of recommendations by the Subcommittee on Stillbirths of the American Public Health Association made at the annual meeting of the organization in Milwaukee, October, 1935. The report of the Subcommittee pointed out that with a falling birth rate and a very slight decline in the maternal death rate, the causes of failure to achieve a surviving child and mother become increasingly important, and that before we can hope effectively to reduce the fetal and maternal losses we must have adequate information with regard to the magnitude of the problem, the variations in prevalence, and the underlying fetal and maternal conditions associated with stillbirth mortality. (See Report of the Subcommittee on Stillbirths in the *Yearbook of the American Public Health Association*, 1935–36, pp. 244–249.)

The plan of the study and the schedule form have been considered and approved by the members of the Subcommittee on Stillbirths of the American Public Health Association, the members of the Children's Bureau Obstetric Advisory Committee, and the Chief Statistician for Vital Statistics of the Bureau of the Census.

Through the cooperation of hospitals with large obstetric services, individual reports are being obtained of all stillborn fetuses of twenty weeks or more gestation that occur in the hospitals during the next few months. The schedule sets forth a definition of a stillbirth which is that recommended by the American Public Health Association in 1908 as amended in 1913. The schedule is an adaptation of the stillbirth certificate proposed by the Subcommittee on Stillbirths of the American Public Health Association in 1935.

WOMEN'S BUREAU, U. S. DEPARTMENT OF LABOR.—At the request of 14 New England organizations, the Women's Bureau conducted a survey of possible industries and services which New England could develop to reemploy many of its jobless factory, clerical, and professional women. The survey was limited to the area in which the known volume of unemployment among women was greatest, that is, the region designated as the "major Boston marketing area."

In order to determine the character and amount of available woman power in Massachusetts, analyses were made of the United States Census figures for 1910, 1920, and 1930 on gainful employment of women, and of the occupations of unemployed women as reported by the Massachusetts Unemployment Census of January 2, 1934. Special studies were carried on in significant

specific localities, in the cotton textile communities of New Bedford and Fall River, and in the shoe manufacturing region of Brockton known as the Old Colony Area. These studies covered the extent of declining production and factory shutdowns in the shoe and textile industries, unemployment in the communities, and the extent to which cotton and shoe workers were registered at the Federal and State employment offices and on relief rolls. An attempt was also made to ascertain the mount of new industry entering these regions and the number and types of jobs opened by these industries.

After ascertaining the amount and skills of available factory-trained women, an analysis was made of the natural resources of New England. On the basis of these findings recommendations were made for new industries which private capital could successfully develop and for the products of which detailed analysis showed large potential markets existing.

Special analyses were also made of the extent of unemployment among domestic employees, clerical workers, and teachers in Boston. The numbers graduated yearly with training in the last two occupations were compared with the numbers securing employment. The tremendous oversupply of women trained in these occupations, together with the considerable number of unemployed household employees, led to a study of potential home services which could be developed to offer new vocational opportunities to girls and women. Analysis was made of the economic status of families living in suburban towns about Boston, and interviews were held with representative mothers as to needed services compatible with the family incomes. On the basis of the findings, recommendations were made for the development of three types of centrally managed household services—child-care services, household service and catering services, and a domestic engineers' household clinic.

The Women's Bureau is also publishing a study of unemployed and destitute women seeking relief. The survey was conducted in 1933 in Chicago, Cleveland, and the Twin Cities—Minneapolis and St. Paul. Incorporated also in the published report are the findings of a similar survey made at the same time in Philadelphia under the direction of Ewan Clague, in which the Women's Bureau cooperated.

Though an investigation was made of the policies and practices governing the administration of relief by the various agencies and of the types of aid given, the published report discusses mainly the results of a study of case records and interviews with unemployed lone women seeking aid. Facts are presented on the women's age, race, nativity, marital status, length of residence in city of present abode, usual occupation, last occupation, reason for unemployment, and relief granted.

OFFICE OF EDUCATION.—*Statistics of Public School Libraries, 1934–35:* The first sending of forms to 6,500 city and county superintendents of schools for a report on public school libraries has yielded a 54 per cent return from the cities and a 22 per cent return from the counties. The second request for reports was made in the last week in March. These reports will give the first

data collected for elementary school libraries by this office. The last collection of data for high school, college, public and society libraries was for 1929.

College Salaries: The March issue of *School Life*, official organ of the Office of Education, contains an article by Henry G. Badger on salaries in privately controlled colleges of 200 to 400 students, 1929–30 and 1934–35. *Circular No. 157* shows salaries paid in 51 Land-grant colleges combined for 1928–29, 1929–30, 1930–31, and 1934–35.

NEW YORK STATE DEPARTMENT OF MENTAL HYGIENE.—The statistical bureau of the Department is cooperating with the National Committee for Mental Hygiene and the United States Public Health Service in a special study of trends in mental disease in New York City. Dr. Benjamin Malzberg is completing a study of trends in mental disease in New York State with special reference to the problem of the increase in the rate of mental disease.

NATIONAL BUREAU OF ECONOMIC RESEARCH, INC.—Dr. Mitchell's annual report as Director of Research this year took the form of a survey of the sixteen years' work: *Retrospect and Prospect, 1920–1936*. Copies were distributed to both contributing-subscribers and *Bulletin* subscribers and may be obtained upon request: 1819 Broadway, New York, N. Y.

The loss of three Directors, two by death: Malcolm Rorty, and Elwood Mead; and one by resignation, Edwin F. Gay, are announced. Meanwhile George M. Harrison, President, Brotherhood of Railway and Steamship Clerks, Beardsley Ruml, Treasurer, R. H. Macy and Company, and Henry Dennison of the Dennison Manufacturing Company have been elected Directors at Large; and Winfield Riefler has been appointed to represent the American Statistical Association.

The first *Bulletin* of the 1936 series was "Income Originating in Nine Basic Industries, 1919–1934," by Simon Kuznets. This report analyzed the incomes (wages, salaries, interest, dividends, entrepreneurial withdrawals) originating in agriculture, mining, electric light and power and manufactured gas, manufacturing, contract construction, railroads, other transportation, communication, wholesale and retail trade. The other four issues included in the 1936 subscription series will be chosen from the following subjects: capital consumption, characteristics of the business cycles, 1927–1933, agriculture and business cycles, wages and hours, security and unemployment, mortgage financing.

MATHEMATICAL STATISTICS AT THE UNIVERSITY OF CHICAGO.—Beginning October 1, 1936, the University of Chicago, through its Committee on Statistics, will provide an expanded program of study in mathematical statistics. The Committee consists of Henry Schultz, Professor of Economics, Chairman; Walter Bartky, Associate Professor of Astronomy; Louis L. Thurstone, Professor of Psychology.

The instruction for which this Committee takes a coordinating responsibility is intended for those who have had the conventional courses in analyt-

ical geometry and in the differential and integral calculus as well as a good introductory course in statistics, preferably one given in the department in which the student intends to do his major work. The courses are divided into three groups:

I. Courses in pure mathematics which are related to statistical theory.
II. Courses in mathematical statistics which are so arranged as to constitute a sequence. These courses are intended to cover the field with sufficient intensity so that the student may gain professional competence in the modern theory of probable inference.
III. Courses in the mathematical and statistical theories of the field of specialization of the student.

Only the second group of courses is under the immediate supervision of the Committee. The first group is given by the Department of Mathematics which has made some re-arrangement of subject matter to suit the needs of the present program. The third group of courses is given by specialists in the mathematical aspects of the social, biological, and other disciplines. These courses are under the supervision of the departments concerned.

For the two years 1936–37 and 1937–38, the courses listed under II will be:

1. Survey of Mathematical Statistics.
   I. The Elements.—Thurstone.
   II. Probability and Least Squares.—Bartky.
2. Correlation and Curve-Fitting.—Schultz.
3. Probability, Sampling, and Frequency Distributions.—Schultz and Bartky.
4. Numerical Processes.—Bartky.

EDGAR SYDENSTRICKER

EDGAR SYDENSTRICKER, Scientific Director of the Milbank Memorial Fund and Consulting Statistician of the United States Public Health Service, died of a cerebral hemorrhage on March 19, 1936.

Mr. Sydenstricker was born July 15, 1881, in China, of missionary parents, the Rev. Dr. Absalom and Caroline Stulting Sydenstricker, both from West Virginia. He came to the United States in 1896. He was graduated from Washington and Lee University, Lexington, Virginia, being elected to Phi Beta Kappa. In 1902, he received the degree of M.A. from the same university. He pursued graduate work at the University of Chicago and Johns Hopkins University. After leaving college, he was principal of the high school at Onancock, Virginia, for three years. In 1905, he became editor of the *Daily Advance*, Lynchburg, Virginia, and soon thereafter also became a special writer for various newspapers and magazines.

In 1907–1908, he was a fellow in political economy at the University of Chicago. Trained as an economist, his early studies, from 1908 to 1915, were made for the United States Immigration Commission and the United States Commission on Industrial Relations. As a special investigator, he made extensive surveys of wages, working conditions, and scales of living of industrial workers, especially in industries employing large numbers of the foreign born.

In 1915, he was appointed the first statistician in the United States Public Health Service and assigned to assist Dr. B. S. Warren. Jointly they investigated the relation of economic status to the health of garment workers in New York City, and made a factual study of sickness insurance as organized and administered in European countries.

From 1916 to 1920, he collaborated with Dr. Joseph Goldberger of the United States Public Health Service in investigations of the causes of pellagra in the South, being in charge of the statistical studies designed to ascertain the relation of dietary, economic, and sanitary factors to the prevalence of this disease in cotton-mill villages. These inquiries proved extremely fruitful and aided greatly in settling once for all the question of the origin of pellagra.

Before the pellagra studies were completed, he was assigned, in the fall of 1918, to work with Dr. W. H. Frost on statistical studies of influenza. A year later, Mr. Sydenstricker was placed in charge of the statistical organization, and Dr. Frost continued to collaborate as a consultant. Other surveys and studies of influenza morbidity followed as new epidemics occurred, and a large number of papers by Sydenstricker, Frost, Collins, Britten, and other associates in the Public Health Service have been published giving a most comprehensive history of influenza from 1910 to 1930.

Continuing the method of field surveys used in the pellagra and influenza studies, Mr. Sydenstricker began in 1921 the now well-known Hagerstown-

Morbidity Survey to obtain complete and exact statistical data on factors related to sickness in a community. This survey provided the first data every assembled on the frequency and causes of illness in an average population group.

For one year, 1923, Mr. Sydenstricker had a leave of absence from the Public Health Service to organize the Epidemiological Service of the Health Organization of the League of Nations. He established a service there which has been continued ever since under Dr. F. G. Boudreau.

In 1925, he became research consultant to the Milbank Memorial Fund. In 1928, he joined the staff as Director of Research and became the administrative head of this Foundation in April, 1935, with the title of Scientific Director. Throughout this period he acted as consultant for the United States Public Health Service.

The broad scope of the public health studies which he initiated or directed in recent years may be indicated by mentioning a few: analyses of health examinations made by the Life Extension Institute to determine the prevalence of physical impairments; special health examinations of a rural population of 1,000 persons; studies of the availability and use of medical and public health facilities; studies of the fertility of different social classes in communities of varying size and of the trends of fertility. In 1935, to answer the much debated question of whether the health of industrial groups was being affected by the depression, he planned a series of surveys to study morbidity and mortality in families of industrial workers in ten communities.

In 1934–1935, he was associated with President Roosevelt's Committee on Economic Security and directed its studies on "Risks to Economic Security Arising Out of Ill Health." Largely as a result of his work with this Committee, a plan for an expanded Federal program of public health and disease prevention was developed.

Mr. Sydenstricker was the author of numerous books, monographs, and reports on various subjects in the field of economics and public health. Among his more important contributions are the following:

BOOKS AND MONOGRAPHS

Brief History of Taxation in Virginia (1915)
Collective Bargaining in Anthracite Coal Industry (1916)
Conditions of Labor in the United States (with W. S. Lauck) (1917)
Hagerstown Morbidity Studies (Collected Papers) (1926–1930)
Health and Environment (1933)

REPORTS AND PAPERS

Study of the Relation of Diet to Pellagra Incidence (Co-authors: Joseph Goldberger and G. A. Wheeler) (1920)
Pellagra Incidence in Relation to Sex, Age, Season, Occupation, and "Disabling Sickness" in Seven Cotton Mill Villages in South Carolina During 1916 (Co-authors: Joseph Goldberger and G. A. Wheeler) (1920)
The Relation of Factors of a Sanitary Character to Pellagra Incidence (Co-authors: Joseph Goldberger and G. A. Wheeler) (1920)

Relation of Family Income and Other Economic Factors to Pellagra Incidence (Co-authors: Joseph Goldberger and G. A. Wheeler) (1920)
Method of Classifying Families According to Incomes in Studies of Disease Prevalence (Co-author: Willford I. King) (1920)
Variations in Case Fatality During the Influenza Epidemic of 1918 (1921)
Heights and Weights of School Children (Co-authors: Taliaferro Clark and Selwyn D. Collins) (1922)
Weight and Height as an Index of Nutrition (Co-authors: Taliaferro Clark and Selwyn D. Collins) (1924)
Disabling Sickness in Cotton Mill Communities of South Carolina in 1917 (Co-author: Dorothy G. Wiehl) (1924)
Measurement of the Results of Public Health Work, An Introductory Discussion (1926)
Declining Death Rate from Tuberculosis (1927)
Tuberculosis Among Relatively Neglected Groups (1929)
Trend of Tuberculosis Mortality in Rural and Urban Areas (1929)
Physical Impairments of Adult Life (Co-author: Rollo H. Britten) (1930)
Physical Impairments and Occupational Class (Co-author: Rollo H. Britten) (1930)
Differential Fertility According to Social Class (Co-author: Frank W. Notestein) (1930)
Study of the Fertility of Native White Women in a Rural Area in Western New York (1932)
Prevalence of Tuberculosis Infection in a Rural Community in New York State (Co-author: Jean Downes) (1933)
Sickness and the New Poor (1934)
How Unemployment Affects Illness and Hospital Care (Co-author: G. St. J. Perrott) (1934)
Health in the New Deal (1934)
The Changing Concept of Public Health (1935)
Medical Practice and Public Needs (1934)
Health Insurance and the Public Health (1935)

In 1917, he became a member of the American Statistical Association, and, in 1922 was honored by election as a Fellow. Between that time and the date of his death, he served on various committees of the Association, and was appointed as a representative of the American Statistical Association on the Social Science Research Council for the period 1931 to 1934. He was elected Counsellor in 1923 and served as a member of the Board of Directors from 1932 to 1934. In 1933, he was appointed Associate Editor, and retained this position for the remainder of his life. His keenness of mind and well balanced judgment always made his confrêres value highly his advice and counsel.

In his relations with subordinates, he was patient, courteous, and considerate, and, in his dealings with others, he was a model of diplomacy. His personality was characterized by an unusual degree of calmness, fairness, and geniality.

He was gifted with the rare combination of imagination and vision in his approach to social problems and of ability to weigh critically and dis-

passionately the facts involved. All his scientific work has been marked by his determination to get at the fundamentals of a problem. If data were not available, he planned to collect them, no matter how arduous or time-consuming an investigation was required. For this reason, most of his studies have been based upon original material and each has made a unique contribution to our basic knowledge concerning the conditions in this country which affect public health.

In brief, Edgar Sydenstricker came as near as a human being can to being the perfect scientist. He always studied his problem till he knew exactly what he was trying to find out. He emphasized thoroughness, but kept a true perspective and hence did not unduly concern himself with irrelevant trifles. He never jumped at conclusions, for he did not allow himself to be influenced by preconceptions or prejudice, and he always weighed the evidence with unusual care. He was a statesman in public health, and all of society was within the field of his vision.

WILLFORD I. KING

New York University
April 24, 1936

NEW MEMBERS

Abrahams, Pauline, 1853-65th Street, Brooklyn, New York

Armstrong, Robert H., Partner, Armstrong & Armstrong, 485 Madison Avenue, New York City

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REVIEWS

Einführung in die mathematische Statistik, by Oskar N. Anderson. Vienna: Julius Springer, 1935. v, 314 pp. 23.60 marks.

This fresh and interesting book on mathematical statistics was written for statisticians, not for mathematicians, by a statistician who considers mathematics as a tool. The author emphasizes this point of view in his introduction, which opens with quotations bearing on this point from Charlier, R. A. Fisher, and Tschuprow, that from Tschuprow being *Statistik spielende Mathematiker können nur durch mathematisch ausgerustete Statistiker überwunden werden.* Various definitions of mathematical statistics are given in the introductory chapter, but the definition towards which the author inclines is "that part of theoretical statistics which is in close contact with the theory of probability and which can almost be called applied probability theory." Under this definition actuarial mathematics could be included, but this subject is considered as too special and really a science by itself.

The table of contents of the book reads very much like that of other works on mathematical statistics. Considerable space in the beginning is devoted to the theory of probabilities. A very readable discussion of the various definitions of probability leads up to the author's own "working definition," as follows: The probability of an attribute in the field of a statistical aggregate is its relative frequency within an aggregate of higher order from which the first aggregate was derived; or, more specially, the probability of an attribute in a sample is its relative frequency in the population from which the sample was drawn. This idea of an aggregate of higher order is a marked characteristic and colors the language throughout the book. As distinguished from the usual notion of "population" with its infinite content, the aggregate of higher order may be finite or infinite.

Professor Anderson suggests six subdivisions of the theory of statistics which a comprehensive book should cover:

1. The study of measures of central tendency and other so-called statistical parameters used for describing a statistical aggregate. This, of course, includes the discussion of averages, measures of dispersion, moments, measures of correlation, index numbers, and so on. Much of this should appear in the more elementary books which the reader of Professor Anderson's book is supposed to have read previously.

2. The study of inferences that can be drawn respecting the average characteristics of an aggregate of lower order (perhaps a random sample) from a knowledge of the corresponding characteristics of an aggregate of higher order (the population from which the sample was drawn).

3. The study of inferences that may be drawn concerning the unknown parameters of an aggregate of higher order from a study of an aggregate of lower order. This part covers the topics of random sampling, "goodness of fit" tests, and Baye's theorem. Professor Anderson's discussion of Baye's theorem is remarkably clear and reasonable.

4. The study of temporal and spatial stability of aggregates of higher orders. A discussion of Pearson's or Charlier's system of curves, Thieles' semi-invariants, and Hermite's polynomials usually would come under this heading, but Professor Anderson touches them lightly for he is more interested in the somewhat unstable aggregates met by the sociologist or economist than in the relatively stable aggregates in biological statistics.

5. The study of aggregates or universes which are unstable in time or space. This covers in part the topics of time series, interpolation, combinations of observations, and the investigation of the existence of stable components of an unstable aggregate.

6. The study of statistical methods necessary to the establishment of relations of cause and effect. This frontier subdivision of statistics is awaiting workers. The attempt to adapt the theory of correlation to use in this foreign field has led to much confusion.

The above six subdivisions of statistical theory are suggested as the content of an extensive and comprehensive treatise. Since the work under review is an introduction into the field, the author selects from this outline those topics which "will give the reader a closer contact with the spirit and concepts of the science." Then, he attempts to make clear to the student that mathematical statistics enables one to secure richer and more trustworthy information from given data than is possible by the so-called elementary methods. Again, mathematical statistics serves another very important function, that of a brake in connection with the wide-reaching conclusions that are often drawn from data meagre in amount and quality. This "Hemmschuh" function is generally unnoticed by enemies of mathematics in statistics. Other influences which dictated the selection of topics were pedagogical and a regard for the mathematical attainments of the reader who is supposed to be pretty well up on his algebra and has a smattering of calculus. In the United States the book would be considered as rather mathematical.

It turns out that the book covers the first three of the subdivisions rather adequately. To do as well for the last three would have doubled the size of the book, so the author was content with discussions in broad outlines with copious footnotes pointing out references in the literature for details or for further study. The footnotes throughout the volume, both explanatory and bibliographical, are a feature and include a well selected bibliography international in scope.

A. R. CRATHORNE

University of Illinois

Elements of Statistics with Applications to Economic Data, by Harold T. Davis and W. F. C. Nelson. Bloomington, Indiana: The Principia Press, Inc. 1935. xi, 424 pp.

There appear to be divergent tendencies among recent textbooks in statistics. One is toward the development in laymen of an appreciation for statistics and an understanding of the rudiments of its methods without a detailed knowledge of mathematics. The other, a manifestation of the de-

mand for better trained practitioners in the field, is toward the provision of a comprehensive background based on rigid mathematics.

This book falls into the second class. The recommendations of the Social Science Research Council Committee on the place of collegiate mathematics in the social sciences have provided the general guides for Professors Davis and Nelson. To study the text effectively a student should have had as a minimum the ordinary college mathematics courses through integral calculus. With judicious elimination the authors suggest that the work can be used by students with less training in mathematics.

The volume is divided into twelve comprehensive chapters: Preliminary Analysis of Statistical Data, Graphical Analysis of Data—Elementary Curve Fitting, Methods of Averaging, Index Numbers, Analysis of Time Series, Analysis of Artificial Data—Probability, Binomial Frequency Distributions, Normal Frequency Curve—Problems in Sampling, Curve Fitting, Elements of Correlation, Multiple and Partial Correlation, and Types of Statistical Series.

That the text covers much ground is manifest in the first chapter. As an introduction to it a history of statistics is given in which in their proper places are mentioned many men whose names do not usually appear in beginning texts: Quetelet, Bernoulli, de Moivre, Euler, LaGrange, Lexis, Walras, Pareto, etc. This introduction makes it clear that the emphasis is that of mathematical and statistical economics. In this same chapter the student is introduced to collection of statistical data, classification, frequency distributions and the binomial theorem. In general the subjects treated are similar to those of the orthodox statistics text. Six kinds of averages are presented: (1) arithmetic mean, (2) quadratic mean, (3) median, (4) mode, (5) geometric mean, and (6) harmonic mean. Three common methods of measuring dispersion are considered: (1) standard deviation, (2) average deviation, and (3) position measures, such as quartiles and deciles. Index numbers are treated "only from the standpoint of the mathematical formula" (p. 114). Time series analysis treats of trends, seasonals, and cycles and, in addition to a general discussion of methods, illustrates short-cut computation devices.

In view of its intended purposes, the first five chapters meet the requirements of the book but are less well done from the point of view of the student than the last seven chapters. Even though a student may have the necessary mathematical prerequisites, these early chapters take too much for granted or depend upon the instructor too greatly. Occasionally, terminology not subsequently used is introduced, and interpretation, so badly needed by beginning students, is not always adequately given, though some sort of an illustration is usually presented.

The superiority of the last seven chapters may be due to the increased need for mathematical treatment in dealing with probability, frequency distributions, sampling, curve fitting, correlation and statistical series—Bernoulli, Lexis, and Poisson. Developments are complete, and description is clear and well done. Even in Chapter XII, which the authors suggest as one of the first sections to eliminate, the treatment is not too involved and is well illustrated.

It is the opinion of this reviewer that Davis and Nelson have provided the best of the mathematical statistics textbooks. At the end of their last chapter the authors say, "The reader is now upon the threshold of modern mathematical statistics and the problems are both numerous and difficult" (p. 333). The truth of this statement cannot be gainsaid, but a student who has passed a course in which this book has been well used will have a much better right and basis to proceed than most students who have "had statistics."

The format and type face combine to produce a very readable text. This reviewer has painstakingly gone through many, but by no means all, of the mathematical and arithmetical developments and has found very few needs for correction. As it is to be expected, there are occasional statements with which one can differ. For instance, "Study of these graphs will enable students to approximate roughly a correlation coefficient from a scatter diagram" (p. 261). It has been the reviewer's experience that considerable practise is necessary, because of the effect of scales, before even close approximations can be made to a correlation coefficient by this device, and that college students do well merely to recognize the presence of correlation from a scatter of observations. Occasionally necessary or helpful qualifying words or statements are not made. In one case a definition is not correct as it is described symbolically (definition of the quadratic mean and its formula, on page 73).

The book has a number of admirable features. Appendix I gives biographical notes on a number of mathematical economists: Cournot, Jevons, Walras, Pareto, and Edgeworth. In Appendix II, there is provided a short review of computation aids, the law of exponents, logarithms, the number "e," and logarithmic series. Appendix III is introduced by a note on interpolation which is followed by tables: five place logarithms, $y=e^x$, $y=e^{-x}$, squares and square roots from 0 to 999, reciprocals, $y=1/\sqrt{2\pi}e^{-\frac{1}{2}x^2}$, area under the normal curve, values of Pearsonian probability, P, and coefficients for fitting straight lines and parabolas. At the end of each section many student problems are given with necessary data provided. Answers appear at the end of the book.

This text is excellently suited for use in institutions where attempts are being made to prepare able practitioners in a rapidly growing field and where students have been well prepared in basic courses.

LESTER S. KELLOGG

The Ohio State University

The Use of Statistical Techniques in Certain Problems of Market Research, by Theodore Henry Brown. Boston: Harvard University, Business Research Studies—No. 12. 1935. iv, 24 pp. $1.00.

This pamphlet consists (1) of solutions of four marketing problems, whose mathematical types are standard in the theory of large samples, (2) a table showing the size of sample to yield accuracy of three times the standard

deviation for values ranging from .1 to 40, (3) notes on sampling problems, and (4) mathematical appendixes. Typical of the problems is one in which it is desired to know whether differences in percentage frequencies of consumer preferences for particular toilet soaps are significant.

The aim of the pamphlet, according to the author, was to provide business organizations with statistical techniques for handling their problems and to facilitate a wider test of the practical applications of these procedures. To a limited extent such aim is accomplished, but it is deeply to be regretted that the author has made several careless or incorrect statements of sampling theory and that he has given no discussion of small versus large samples. Illustrative of the former point, we find him stating on page 16, "Experience shows that for practical purposes it apparently is safe to say that three standard deviations will be exceeded only once in 100 trials," whereas tables of the probability function give the figure of 3 in 1,000. In another instance, p. 8, he writes (the reviewer's italics): "The method of calculating this value of σ_s is derived directly from the *definition*, which states that the standard deviation is equal to the square root of the average square of the given values *corrected* by the square of the average of the values." Moreover, the mathematician will be somewhat amused in reading the passage on page 7, "The data which are gathered by this process vary from observation to observation. They are known, consequently, as variables." With respect to the issue of small and large samples, the author says nothing but lists in his table of size of sample, values of n considerably less than one hundred, a commonly accepted point of demarcation. Nevertheless, the study should prove of value to organizations meeting the types of problems considered.

CHARLES F. ROOS

Cowles Commission for Research in Economics

The Application of Statistical Methods to Industrial Standardization and Quality Control, by E. S. Pearson. London: British Standards Institution, Publication No. 600. 1935. 161 pp. 5s. 6d.

Largely through the work of Dr. W. A. Shewhart, of the Research Laboratories of the Bell Telephone Company, there has resulted within the last few years a marked increase in interest among representatives of certain manufacturing industries, here and in England, in the application of statistical methods to problems of standardization and specification. This publication is a part of a program initiated by Shewhart, E. S. Pearson, B. H. Wilsdon, and others at a conference held in 1932 under the sponsorship of the British Standards Institution.

The book is essentially a review of the simpler statistical methods applicable to the problems of standardization and specification of quality of manufactured products. With a minimum of technical terminology the author has clearly and systematically shown why statistical technique is essential in the solution of many industrial problems of control of quality and has discussed the type of assistance it can give. It is shown that the actual framing

of specifications depends, in many cases, upon preliminary investigations along statistical lines. Considerable emphasis is placed on the method of sampling as a basis for securing conformity to a specification, by which the consumer can be assured of quality on the one hand, and the producer warned against lack of control of quality on the other. "Fiducial limits" rather than the traditional "probable error" are used for expressing the accuracy of the information deduced from the sample. In situations where it is not economically feasible for the consumer to test a sufficient number of units to give him information of the desired accuracy regarding the consignment, the problem arises of devising a method of assessing the level of the control of quality during routine production, to be furnished by the producer under a guarantee or mark system. Methods of collection and arrangement of test records on control charts are considered for handling this type of problem. The author has limited his treatment to the case of one statistical variable and has only briefly mentioned the possible value of the correlation or regression method in specification and control problems.

The methods and discussions are thoroughly illustrated by several sets of data from various principal industries. The book is well equipped with tables, figures, and folded charts and contains seven appendices of a slightly more technical nature. Because of the simple and systematic form in which Professor Pearson has presented the material in this book, it should arouse further interest in the application of statistical methods on the part of manufacturers and others concerned with problems of specification and standardization. More specifically, it should prove useful as a guide for the non-mathematical technician who must sooner or later learn to recognize, among the various problems that arise in his immediate domain, those which call for treatment by statistical methods.

S. S. WILKS

Princeton University

Manuale di Statistica, by Felice Vinci. Bologna: Nicola Zanichelli. 1934. Vol. I, vii, 230 pp.; Vol. II, 302 pp. Lire 25 each.

Statistica, by Filippo Virgilii. Milano: Ulrico Hoepli. 1934. xii, 266 pp.

Felice Vinci offers in his two volumes a comprehensive presentation of methods of statistical analysis in its application to the study of social phenomena. Theoretical discussion and examples of actual computations are combined in such a way as to give the reader not only fundamental understanding but also practical guidance in empirical work.

The First Part (Vol. I) is devoted to problems of tabulation and graphic presentation of primary data, to discussion of characteristic constants and a very useful, because careful and explicit, description of various methods of empirical curve fitting. The Second Part (Vol. II) contains a rather minute analysis of a very complete array of different theoretical distribution schemes. Part Three (Vol. II) is entitled: "Relation Between Groups of

Observations." The somewhat heterogeneous contents of these chapters include the discussion of such subjects as index numbers, mortality tables, time series analysis, correlation, and, finally, three short but very suggestive studies in economic dynamics.

Built on rather conservative, one might say classical, lines, the work does not contain any reference to the recent developments in the theory of sampling.

Professor Vinci carefully avoids the use of infinitesimal calculus. Thus the majority of his mathematical proofs is given in a somewhat lengthy form of algebraic developments. In a few instances, where this method ceases to be useful, the argument is interrupted and final formulae are presented without formal proof but with appropriate interpretations.

Although every opportunity is used by the author to derive the empirical examples from the realm of economic phenomena, a very large part of his factual material is still drawn from the traditional fields of biometrics and demography. Numerous bibliographical notes and an extensive index make of Felice Vinci's *Manuale* a valuable reference book.

Filippo Virgilii's *Statistica* is a very elementary introduction expressively written along the lines of the "official program" for use in technical schools. A large part of this diminutive volume is given to a discussion of the nature of statistical science and of the general principles of induction. In the following methodological chapters the space is equally divided between an exposition of elementary principles of tabulation and classification of primary data and explanation of fundamental properties of the main statistical constants. The rest of the book is devoted to what might be called an abbreviated statistical abstract, a quantitative description of the economic, social, and cultural life of Italy.

WASSILY LEONTIEF

Harvard University

Public Intelligence, A Study of the Attitudes and Opinions of Voters, by Seba Eldridge, Lawrence, Kansas: University of Kansas. Humanistic Studies. Vol. V, No. 1. 1935. 101 pp. $1.00.

Twelve hundred fifty questionnaries on the League of Nations, the tariff, and compulsory arbitration of industrial disputes were circulated, chiefly in Kansas, during the years 1924–1927. The questionnaire was in the form of a true-false test, and scores of political intelligence were assigned on the basis of the agreement of the voters with the consensus of informed opinion on the issues covered. The data were subjected to detailed and competent statistical analysis, the nature of which will be apparent from a brief summary of some of the principal results.

"On a true-false test assumed to represent a fair measure of political intelligence, a mean score of 21.4 (on the basis of 100 as perfect) was obtained by the group of 1250 considered as a whole; 24.4 per cent of this group had scores of 50 or above (the assumed index of civic competency); while 20.4

per cent received negative scores, indicating that voters had to this extent been conditioned to a larger number of erroneous than of valid opinions on public questions" (p. 88). Although there is no pretense that the sample is either adequate or representative for the country as a whole, there is considerable probability of its representativeness for each of the occupational groups included. The fact that professional workers are greatly over-represented, whereas farmers and housewives are under-represented, suggests, however, that a representative sample would show very much worse results than those indicated above. Professional people averaged highest; farmers, non-manual workers, and business proprietors lowest. Housewives and manual workers occupied an intermediate position. No important differences on the basis of sex were observed. Schooling above high school is positively correlated with political intelligence. Reading of daily papers is of doubtful significance, but political intelligence is directly and significantly associated with the number of magazines read regularly, especially when the magazines are the better liberal journals. Multiple and partial correlations show magazine reading to be second in significance only to college education among the measurable factors. Age is found to be of no significance when other factors are held constant. Spatial, occupational, and partisan mobility are found to be positively associated with political intelligence.

The general significance of this excellent monograph lies perhaps chiefly in the methodological transition which it represents and which has been carried still further in the recent work of Thurstone and Gosnell. These hundred pages are of more scientific relevance and value than the last dozen volumes I have seen on the same subject by conventional political scientists. Especially praiseworthy is the author's detailed account of his methods and his recognition of the limitations of his study.

GEORGE A. LUNDBERG

Columbia University

Analisi di Statistica Economica, by Libero Lenti. Milan: Casa Editrice Aracne. 1934. 166 pp. Lire 15.

As the author points out in the preface, the volume under review is primarily descriptive in character. Its main purpose is to present a discussion of some of the significant statistical sources, materials, and techniques which may be used to advantage in a study of Italy's economic life during the post-War period. The materials contained in his book, Dr. Lenti suggests, will probably be used by him at some later date as a basis for a more comprehensive and detailed investigation in this field.

The volume begins with a brief introductory discussion of the techniques evolved for the description and measurement of economic phenomena and with a short critical analysis of some of the better known methods of economic diagnosis and of economic forecasting. This analysis is followed by

the presentation of actual statistical materials on some of the major phases of Italy's post-War economic activity. Throughout this presentation the author interpolates detailed methodological discussions ranging from a critical appraisal of statistical sources to an analysis of the proper formulae and methods of weighting employed in the computation of some of the indexes which he presents in the volume. Special chapters are devoted to the treatment of such topics as the construction of indexes of Italian stock exchange prices, of Italian clearing house operations, and of the trading volume on Italian stock exchanges, and to the statistical measurement of money market rates, of the activity of Italian corporate enterprises, of wholesale and retail prices, of bankruptcies, of the volume of railroad and building operations, and of unemployment.

Although from the point of view of statistical technique Dr. Lenti's book offers little that is novel to the trained statistician, the materials on some of the more important phases of Italy's economic life and the accompanying methodological discussions which the volume contains are interesting and instructive. Even though Dr. Lenti's principal objective in writing this book was admittedly that of presenting, mainly for the benefit of Italian economists, an elaboration and methodological interpretation of significant statistical data, this reviewer feels that some discussion at least of the light which these data throw upon Italy's economic position during the depression and upon the economic condition of her people would have been of interest both to the general reader and to the trained economist for whom the volume is primarily intended and should therefore not have been entirely omitted.

William G. Welk

The College of St. Thomas

New Numbers, by F. Emerson Andrews. New York: Harcourt, Brace and Company. 1935. 168 pp. $2.00.

My task as a reviewer is neither to defend the system in existence nor to be an exponent of a new system. The author evidently is not a mathematician. He is an enthusiastic supporter for the number 12 as the base of our number system. My job is to determine whether the author has fairly and accurately stated his case.

The title is misleading. The numbers are not new. The idea is not new. Most texts on algebra that give more than the elements of algebra contain chapters on expressing numbers with a base different from 10. And then on p. 15 the author states, "Yet, because this idea questions the habitual, it has very seldom been tried, even among mathematicians." At the bottom of p. 32 the author states "There is no way of packing ten eggs . . . except in two rows of five eggs each . . . either way the package is long and unhandy." It hardly seems possible that the author has not seen the carton

now used for handling eggs—two rows of six eggs. No doubt he does not approve of this package which is in well-nigh universal use.

On p. 33, the author says "We divided time almost happily." Is it possible that in his enthusiasm over revising our number system that he has not heard of the dissatisfaction with our calendar?

The author's wording is not accurate. As an example, on p. 46, "Table I lists the actual factors . . . and gives the number of factors." This is not true. From Table I by counting one can determine the number of factors, but the table does not give the number of factors. This may seem to be quibbling, but the example is just one instance of the author's inability to state his ideas clearly and accurately. I pick this example, for it is simple and obvious. Again, on p. 45: "From the viewpoint of ease of calculation, it would be stupid to select for our base any number which does not appear frequently in such computations as we have examined; it would not be apt to come out in round numbers often enough." Is it clear what is meant by the last *it*? The cited examples are typical of the poor quality of the expression throughout the book.

The book is crowded with bald assertions for which no proof is attempted. For example, p. 49: "An examination of 6 reveals real possibilities. It is at least far better than our present number base." "To learn 12 tables when our number system is based on 10 was . . . an open acknowledgment of the superiority of 12."

P. 58: "In the system of 10, 33 of the totals end in zero. In the system of 12, 40 of the totals end in zero—another advantage in practical brevity for the new radix." This statement contains two errors: 33 and 40 should be replaced by 32 and 39. The author seems lacking in even the elements of logical thinking. For example, on p. 48, after some rambling statements he inserts a *therefore*, intimating that he has proved something. The statement just quoted from p. 58 might with equal logic read as follows: In the system of 10, 9 of the totals end in 5; in the system of 12, 3 of the totals end in 5—another advantage in practical brevity for the old radix.

The argument on p. 69 is not fair. In solution A the first half is done with a base 10; the second half is a mixture of bases 10 and 12. Solution B is done wholly with a base 12. In the numerical example on p. 74 the two bases are again mixed in a similar way in one solution—and the second solution is done wholly with a base 12.

The author undoubtedly is not a mathematician and is not acquainted with the accepted language used in the most elementary high school algebra. Otherwise he would not use the expression, "The behavior of exponents when they themselves are raised to powers." The accepted wording is "power of a power."

I do not find anything new in the book—not even a new way to make errors—not even a new way to reason illogically.

W. V. LOVITT

Colorado College

On Economic Planning, Papers delivered at the Regional Study Conference of the International Industrial Relations Institute (IRI), New York, November 23–27, 1934. Edited, with an Introduction, by Mary van Kleeck, and Mary L. Fledderus. New York: Covici Friede. 1935. xiv, 275 pp. $3.00.

This book is alleged to "represent the first serious attempt to make a comprehensive study of the subject of social economic planning." The Conference, of educators, technicians, economists, engineers, and public officials, was called to consider this theme: "What kind of economic planning can end unemployment, establish security, and raise standards of living in proportion to productive capacity?" The answer in this book is that it is possible to have economic planning in a fascist society, in a communist society, or in a democracy. Very largely in a praiseworthy attitude of impartiality, the various papers discuss the different kinds of problems economic planning has faced in these various types of social organization. It is true that the discussion of the possibilities of economic planning under fascism so far as they point towards general welfare receive little sympathy; and it is true that the description of the theory of the economic planning of Russian communism is presented in such a manner as to ignore the costs involved in the various forms of liquidation which have taken place there. But taken as a whole, the book represents the view that economic planning is a term which can be used synonomously with governmental intervention and to the extent that there is governmental intervention there is economic planning. Thus it is not the conventional idea that economic planning is an alternative form of social organization, of which we can take none or all.

Such a conception is clearly brought out in the discussion by Harold Butler, director, International Labor Organization, on "Economic Planning and Labor Legislation," chapter ii of the book, where he says: "In Great Britain, payrolls fell to a relatively moderate extent, and the money volume of retail sales was sustained. There is a reasonable supposition that the heavy falls (in retail sales and payrolls) in Germany and the United States were correspondingly responsible for the movement of production in those countries. One can scarcely doubt that the form of distribution of the purchasing power of a country, such as is involved in unemployment benefits and assistance, workmen's compensation, old age pensions, and other forms of social insurance, played a tremendous part in maintaining an even keel of economic activity." Again, he says: "It was at a very early stage of industrial history that social and humanitarian considerations led governments to impose these limitations on the so-called pure process of economic readjustment. That ideal society in which conditions of labor were determined exclusively by the forces of supply and demand has really never existed." Finally, he says, "it is quite obvious from what has been said that economic planning and labor legislation are not conditioned, in any inevitable fashion, by any special type of political institution for their realization."

Valerian V. Ossinsky, Vice-Chairman, State Planning Commission (Gos-

plan); and director, Central Administration of National Economic Accounting of the Union of Soviet Socialist Republics, discloses a liberalism which is quite unexpected. Lenin's belief that Russia could attain socialism without necessarily carrying the class-war into capitalistic Europe or America, and also the fact that the material aspects of soviet planning have been so amazingly successful may be considered to account for this new liberalism. Engaged as he is in the practical operation of a program to bring about production, his whole attitude towards the Marxian formulas seems to be not one of the doctrinaire, but a sort of presumption in favor of the ideology that the aim (which is their primary aim) of a planned economy may best operate if the society is made classless because society can then be functional. This is a far cry from Marx and the dictatorship of the proletariat.

In all these respects, Ossinsky, the practical worker in a socialistic state, is in striking contrast with the doctrinaire Alfons Goldschmidt, whose discussion constitutes the opening chapter of the book. Here we have the old formulas, decorated with a bewildering set of "laws" of which Dr. Goldschmidt is an incorrigible formulator. If stripped of his digressions into the formulation of cryptic "laws" of this and that, Dr. Goldschmidt's theoretical discussion of economic planning is a searching one. But in spite of his critical review of the history of utopias, he is himself a sort of utopian Marxist. Dr. Goldschmidt thinks Russia is an industrial democracy. Perhaps it is, but if it is, it is not Marxian socialism but Fabian socialism.

Walter N. Polakov discusses in chapter xiv the unused productive and technical capacity in the United States, using the figures of the National Survey of Potential Product Capacity. This last study was made, says Dr. Polakov, with a view to determining what consumer goods and services could be expected if production were directed toward the satisfaction of needs and reasonable wants of the population and if it were limited by physical factors only. The figures thus studied showed that in 1929 there could have been produced 84.8 per cent more than was produced. When this figure is reached, it becomes obvious that the NSPPC ignored not only production for profit but also the human factor. In 1929, whether the system was operating for profit or not, there was not sufficient labor, and especially trained labor, to produce according to the NSPPC figures. In general, the argument presented by Dr. Polakov is like assuming that there is "waste" capacity *necessarily* if there is excess capacity; and yet it would be a sadly short-sighted type of economic planning (even when done for use and not for profit) which did not build and equip plant and machinery capable of meeting a growing market during the life of that material equipment.

A general summary of the technological problems of economic planning are discussed in an excellent final chapter of the book by Mary van Kleeck, the full implications of which should be thoroughly studied by all "economic planners."

The liveliest chapter in the book is the one written by Earl Browder, in which he attacks all the capitalistic, New Deal, Fabian socialistic, technocratic, and EPIC types of planning. Going back to first principles, which to

him are those of Marx and Engels, he quotes extensively from the *Communist Manifesto* and from Lenin. Standing foresquare on his Marxian tenets he proclaims the forthright revolutionary doctrine that the only solution is the dictatorship of the proletariat. There is no pussyfooting here—if for no other reason, this book is worth having because it contains Browder's vigorous presentation of the views of the fighting communists.

JAMES G. SMITH

Princeton University

Economic Planning and the Tariff: An Essay on Social Philosophy, by James Gerald Smith. Princeton: Princeton University Press. 1934. x, 331 pp. $3.00.

This work is an analysis and criticism of what is called economic planning, one phase of which is a tariff policy looking toward a large measure of national self-sufficiency. General economic or social planning, according to the author, "means that in this age of statistics and scientific methods we should be able to diagnose with relative precision the state of the national and world economy. It means also that because of our vast quantity of reliable statistical facts we can either *supplement* economic forces or *substitute for them* a planned direction of the economy." It is pointed out that since the World War social planning in the United States has embraced such projects as prohibition, helping European countries to stabilize their currencies and stay on the gold standard, aiding monopolistic enterprises in their price-stabilizing efforts, and raising still further our already high tariff by the Hawley-Smoot Act of 1930. The effect of the first has been a "wide-spread expansion of racketeering," and of the others, "an inflated credit structure, attempted stabilization of agriculture in this country, . . . a price structure in industry honeycombed with monopolistic stabilization schemes," and the growth throughout the world of retaliatory tariffs.

In attacking the protective tariff system the author has some illuminating chapters on the hidden costs of economic nationalism, its financial costs, contemporary fantasies of the economic nationalist, the bewilderment of public opinion, and recent efforts at tariff reform. Much of this material, though interestingly presented, covers ground with which students of international trade are familiar. The effect of tariff barriers on wage rates receives considerable attention. The absurdity of the contention that a protective tariff is necessary for the maintenance of the American standard of living (commonly known as the pauper-wage argument) is well exposed. The author, although disclaiming to be a free trader in the sense that tariff barriers should never be employed under any circumstances, sees in their general levelling an important step not only in trade revival but in the maintenance and spread of general prosperity.

The argument of the book is in substance that interference with the operation of competitive forces, whether by government or by special interests, is socially harmful. This does not mean the adoption of a pure let-

alone policy on the part of the government, but rather an "adequate use of its police power in the enforcement of conditions of free economic markets." Otherwise "competitive capitalism ceases to operate."

There is much sound criticism in the book of certain phases of what is called "economic planning." No attempt at a purely artificial control of prices, credit, and the like, such as has characterized much of our governmental efforts to aid business or relieve the economic distress of large classes of our people, can be permanently successful. Economic as well as physical relationships are governed by laws. One may question, however, such a statement as this: "*The business cycle is a product of human manipulation through governments either as a result of common blundering, or as a result of control by special interests.*" (Page 162; the italics are the author's.) Even though we may grant that the influence of "human manipulation through governments" has played a large role in our major depressions, the maladjustments resulting from variation in the relative mobility of labor and capital, and from failure of business organization to adapt itself readily to a changing technique, have been fully as potent a factor in bringing about the industrial changes which characterize the business cycle. Furthermore, unregulated competition in the utilization and exploitation of raw materials, even where there is no danger of monopoly, can be provocative of serious social waste. The regulation of competition in the interest of fair trade practices and in behalf of the conservation of natural resources do involve some measure of economic and social planning. Barring these and some other criticisms that may be made, the book is a valuable and needed contribution to the literature of an age of social experimentation and economic nationalism.

ABRAHAM BERGLUND

University of Virginia

Prices in Colonial Pennsylvania, by Anne Bezanson, Robert D. Gray, and Miriam Hussey. Philadelphia: University of Pennsylvania Press. 1935. xix, 445 pp. $4.00.

This volume is No. XXVI of the Research Studies of the Industrial Research Department of the Wharton School. It is a thoroughly competent piece of work and should be of great value to students of price history and of colonial history, for it is more than a history of prices; it is almost a history of commerce. The authors have accumulated a mass of interesting information concerning the prices of the chief articles of trade in Philadelphia and the causes of their fluctuations. Their chief conclusion is that there were nine distinct cycles in prices between 1720 and 1775, of which some can be explained by wars and others cannot be completely explained. They have also established the existence of marked seasonal fluctuations, and of an upward trend in most of the prices studied, which was more rapid after 1744 than before.

In general their conclusions agree fairly well with those of other students of 18th century colonial prices. They have prepared an index of the wholesale prices of twenty commodities by months, which is without doubt the best price index that has yet been published covering that period. The peaks of price cycles shown by the index usually differ by a year from those reported for New York and South Carolina, sometimes leading, sometimes following. Similarly they differ from those found for consumers' goods in London by the present reviewer.

Likewise there is some difference between the trend shown by this index and that shown by others. From 1720 to 1774 this index rose 57 per cent, and a supplementary index of 12 commodities rose 48 per cent. Warren and Pearson's index of 10 commodities in Philadelphia rose 80 per cent in the same interval. As they were all unweighted arithmetic averages, the differences between them are partly due to the choice of commodities, and partly to the use of different base years. Stoker's indexes for New York rose 38 per cent from 1720 to 1774. Geometric averages would have shown less of an upward trend and less difference in the slope before 1744 and after. The authors of the present study did also calculate geometric averages for the period from 1731 to 1774. On the other hand, the use of weights in calculating the averages, although perhaps reducing the upward trend over the whole period, would probably have accentuated the contrast between the earlier part of it and the later, since the commodities customarily given the heaviest weights were the ones that showed this contrast most plainly.

The authors used an unweighted index because of the impossibility of deciding what weights would be correct. They also limited their index to those commodities for which monthly figures were available without too many gaps.

As they point out, the trends of the commodities studied were not alike. Turpentine, bar iron, tea, and loaf sugar declined; pitch, tar, salt, rice, pepper, gunpowder, and linen cloth had horizontal trends; and grains, meats, cotton, molasses, brown sugar, wire and staves rose. Is it not possible that we have here an illustration of the law of diminishing returns applying to agricultural products as the population increased, while at the same time improvements in manufacture and ocean navigation reduced the cost of manufactured goods and imports from Europe and the Orient? Most prices shared in the cyclical swings shown by the composite index, but their different trends make the index, like all indexes of wholesale prices of raw materials and imported goods, an unreliable basis for deductions with respect to trends in the purchasing power of money.

It is also of interest to note, in view of the present-day discussion of "administered" prices, that even in the 18th century the prices of domestic manufactured products, such as loaf sugar, rum, and iron, fluctuated less frequently and less extensively than those of raw materials. Did they have trusts in those days, or was it merely due to the fact that wages of hired laborers are an important element in cost and that they do not react promptly to booms and depressions?

If this price index is to be used only to establish the existence and timing of business cycles its construction is wholly justified. If it were to be used to measure trends in the purchasing power of money, weights would be essential, and it would be necessary to include many more commodities, especially manufactured goods, and services as well. The authors apparently realize this limitation and have announced their intention to publish studies of retail prices and wages. Perhaps when they do so they will attempt to construct an index of the cost of living of the working classes, and of real wages. The purchasing power of laborers is at least as important a subject for statistical research as the cyclical movements of wholesale prices, although it has received less attention from statisticians and price theorists.

R. S. TUCKER

Westfield, New Jersey

Das Problem der langen Wellen und die Entwicklung der Industriewaren-Preise in den Jahren 1820–1933, by Juergen Kuczynski. Basel: Philographischer Verlag. 1934. 29 pp.

Weltproduktion und Welthandel in den letzten 100 Jahren. Statistische Studien, insbesondere zur Entwicklung unter dem Monopolkapitalismus, by Juergen Kuczynski. Liepāja (Libau): Verlag der Buch- und Steindruckerei Gottl. D. Meyer. 1935. 72 pp.

The results of Kondratieff's famous investigation on long waves found a surprising echo in the international literature. His theoretical assumptions were weak and the statistical methods applied rather crude, but in spite of these deficiencies, which Kondratieff himself admitted, many economists of reputation speak of long waves as established facts. But do they exist at all? Dr. Kuczynski denies it categorically. In his opinion it was a great mistake of Kondratieff to include in his indices agricultural prices, since the phenomenon to be explained is specifically industrial. Excluding all commodities that were not of pure industrial origin, Dr. Kuczynski obtains fluctuations which do not show any long waves. He takes international indices and then indices separately for four leading industrial countries, namely England, United States, France and Germany. In both cases he examines in every cycle the absolute and the per cent deviation from the cycle average. The deviations do not show any regularity. Sometimes the direction is changing from cycle to cycle, sometimes the direction remains the same in two consecutive cycles, but except for France, the country of least developed industry, the price fluctuations reveal no waves of fifty to sixty years as Kondratieff seems to have found. As Spiethoff has shown, prior to Kondratieff, in some periods longer and more severe depressions prevail; in others, longer prosperities and milder depressions. This is a fact that can not be ignored. Dr. Kuczynski should therefore have examined more carefully the behavior of prices to see whether or not these fluctuations have something to do with the structural change in the type of business cycle described by Spiethoff. The existence of long waves may be question-

able, but the problem raised by Kondratieff has not been solved by Dr. Kuczynski, although his scepticism will be wholesome for further investigations.

Dr. Kuczynski's second book does not present a uniform, well balanced investigation; it is more a collection of a few papers, partly published before in the "Konjunktur-Statistische Korrespondenz," Heft 4/5, 1934. They are however united by one leading idea: to show statistically the retardation in the expansion of the capitalistic system. The author makes a distinction between two—in his opinion—entirely different periods in the industrial evolution during the last 100 years. The first period—up to the crisis of the 70's—he characterizes by free competition; the second one by growing cartellization and monopolization, which became more pronounced in the last decade of the 19th century. In the first period he finds for production a strongly increasing rate of growth, in the second a decreasing rate. Dr. Kuczynski reaches his results by examining the development of production in England, Germany, the United States, and France. He analyzes the trends in these countries, first, separately for each, and then he compares the joint trend of the four countries with that of total production of the world. The indices he analyzes do not show any long waves. The decline in the rate of increase since 1900–1909 is, according to Dr. Kuczynski, due primarily to the paralyzing influence of the growing monopolistic organization.

Examining the growth of international trade, Dr. Kuczynski comes to similar results; but here he sees the main causes in the following three factors: (1) the development of national industries, (2) the exhaustion of virginal markets, and (3) the retardation of industrial expansion.

The concluding three brief chapters, which can be considered only as an annex, deal with the problem of productivity and insufficient utilization of capacities in the United States and the problem of unemployment in Germany and the United States.

To summarize Dr. Kuczynski's results in a few words: The capitalistic system is no longer able to master the production forces on the scale of previous decades. The ideas are of course not new, neither is the statistical approach; but Dr. Kuczynski carried through his analysis with greater penetration than it has been done before. The author is aware of the theoretical and statistical pitfalls he is facing, but he underestimates the difficulties the problems involve. Does there exist a general law of industrial growth, and if it exists is it possible to prove it statistically? What is capacity from the strict economic point of view? Does there exist any possibility of measuring the influence of monopolies on insufficient utilization of means of production? Is not utilization a very complicated phenomenon attributable to different causes, which must be carefully distinguished? These and many other problems Dr. Kuczynski did not even raise. His general statement about monopolistic organization is to some extent true, but his analysis does not add very much to establish it statistically.

EUGEN ALTSCHUL

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Fluctuations in American Business 1790–1860, by Walter B. Smith and Arthur H. Cole. Cambridge: Harvard University Press. 1935. xxix, 195 pp. $5.00.

The authors of this study hope that they will find in their audience the student specializing in business cycles, but they intentionally attempt no better solution to the problem of the business cycle than to present evidence of "the persistence of the sequence in movement of stock values, commodity prices and interest rates." Undoubtedly, the additional material tabulated in the appendices should delight the heart of any economic statistician and should aid in future analyses.

As a matter of convenience, the period under study has been divided into three sections, namely, 1790–1820, 1820–1845, and 1843–1862. There is a certain necessary overlapping in the statistical series, since the composition of the various indexes is dissimilar for the periods. The first section has been analyzed by Dr. Smith, and the last two sections by Dr. Cole. Each section is prefaced by a general statement of the economic setting for the period, followed by chapters dealing with commodity prices, security prices, and banking. In the last two sections, the discussion of public land sales is included in the chapters dealing with stock prices, and there are also chapters dealing with the volume of trade. The final chapter of each section is a general summary based on the graphic presentation of various statistical series. This method of subdivision aids the reader materially in following the authors through a maze of complex movements without losing the sense of relationship among the series. It is impossible, however, for the reviewer to accept the implications and the conclusions of the authors in all cases. In the comparison of stock market declines of the period under study with the decline of 1929–1932, they suggest that "an index of crisis severity might be constructed in which degree of movement was weighted by its duration" which would indicate that the "decline which in severity most nearly equaled (or perhaps exceeded) that of the 1930's occurred nearly a hundred years previously." Such an index attaches no significance to the volume of trading and the wider breadth of the recent markets. Likewise, in the discussion of commodity price movements, there is the hint of favoritism for "their" indexes.

A point of major disagreement is the "catastrophe of 1837" when a "summer's fever is seen to be but a minor part of an eight-year sickness," so stated by Dr. Cole. The swing in commodity prices is used as the evidence of changing business conditions, and the period 1830–34 is said to approximate closely the "business cycle." Here the index swings from 82 to 99 and back to 86. The index then reaches a maximum of 131 (February, 1837), drops to 98 (September, 1837), to rise to 125 (February, 1839). The statement "that there was a broad upswing of price from 1834 to 1839 with a brief and temporary reaction in 1837" is unconvincing in view of the statistical evidence. Not only does Dr. McGrane's *The Panic of 1837* indicate a depression in 1838, but this view is supported by *Business Annals*.

The reader, however, can still interpret the movements for himself, because the authors are not dogmatic and suggest future attempts at theoretical analysis. The study includes 75 tables and 48 charts. Studies of this type are valuable, leading to ultimate agreement among students of business cycles.

WALTER A. BAUDE

University of Cincinnati

World Finance 1914–1935, by Paul Einzig. New York: Macmillan Company. 1935. xvi, 370 pp. $3.00.

Various aspects of the controversy over inflation and deflation are aired in a highly provocative style in a recent book by Dr. Einzig. This is one of a long series of books he has published in the field of international finance and shows again his wide knowledge both of detail and of broad questions which are of urgent present concern.

This recent volume is a summary and recapitulation. It covers in rapid survey the entire post-War period and touches on scores of crises, conferences, and agreements. These events are made the vehicle for a persistent and unremitting attack on those who have favored deflation, who aimed at a return to an old style "normal," and who have believed that money stability in the classical sense was important to economic prosperity.

The discussion of *reflation*, and the passage in Chapter 3 in which "bank rate worship" is discussed with keen irony give an indication of almost complete rejection of the century-old ideas which have determined many central banking policies. In pushing to an extreme in some cases Dr. Einzig leaves the reader somewhat at a loss as to what he would substitute for familiar financial traditions and authorities in the event that they are completely discredited by past or future crises. He does not state a clear preference for dictatorship, communism, or the "planned economies" put forward by many "left-wingers." He even takes care to indicate that the student of the year 2000 will find the truth somewhere between two extremes. Nevertheless he uses historical incident to make more devastating his attack on the deflationists and implies at the same time that only inflation can be controlled, constructive, and rich in economic achievement.

For some students the scope and inclusiveness of the book will recommend it especially for an introduction or review. Those who come to the field without a broad knowledge of the varied happenings in currency and finance in post-War Europe may well find this book a preliminary to their further studies. To others seeking for unrevealed information and sidelights on important happenings there will be some disappointment, because this book has less new information than Dr. Einzig's former studies. It is interpretative in a general way and does not put forward much information not already available in various scattered sources.

In this case, and in other instances, Dr. Einzig has done valuable work in broadening the horizons of many who might otherwise fail to see the

far-reaching implications of financial actions or who are inclined to stress two-country, to the exclusion of more complicated, financial relationships. For instance, in describing the devaluation of the pound sterling in 1931 he places the episode in its complex setting and shows how many varied forces played upon the British currency and pressed it down. Even in giving the history of these critical events, of which he had intimate knowledge because of his many contacts in the city of London, he fails to answer some of the basic questions that still puzzle many economists. He does not clear the uncertainty in some quarters as to how short-time capital movements and the German credit situation could reverse so quickly the studied opinion of British finance emphatically stated in June, 1931, in the Macmillan Report and bring a complete shift in policy and in the position of the world's first currency.

ELEANOR LANSING DULLES

University of Pennsylvania

Public Works in Prosperity and Depression and Their Utilization as an Agency of Economic Stabilization, by Arthur D. Gayer. New York: National Bureau of Economic Research, Inc. 1935. (Prepared for the National Planning Board, Federal Emergency Administration of Public Works.) xx, 453 pp. 100 tables, 3 charts. $3.00.

The book attempts a two-fold attack upon its problem; one, the examination of factual evidence, and the other a consideration of questions of theory and policy.

The greatest contribution this book makes to the statistics of public works and construction is its revision of the series of estimates of the total dollar volume of construction, public and private, presented in an earlier publication of the National Bureau of Economic Research, *Planning and Control of Public Works*, by Leo Wolman, assisted by Dr. Gayer. The estimates cover the period 1923–1932 and provide what is probably the most satisfactory comprehensive measure of the volume of construction which has been compounded out of the meager statistics available.[1]

Although such an estimate of total construction values is extremely useful, the inadequacy of the data upon which any such estimate must be based should be recognized. For instance, public construction is estimated to have risen from 2.97 billion dollars in 1926 to 3.71 billion in 1927, an increase of about 25 per cent. This increase was sufficient to offset a considerable decline in the estimated private construction and to cause a rise in the grand total. An examination of the constituent figures on public construction, however, reveals that 460 of the 740 million dollars total increase are accounted for by an estimated increase in expenditures for construction and maintenance of city streets and bridges, which is based upon F. W. Dodge

[1] The Gayer estimates have been projected through the year 1935 in an article by Lowell J. Chawner, "Construction Trends and Related Factors," in the April, 1936, issue of the *Survey of Current Business*.

Corporation figures covering only 14 cities.[2] City expenditures data given elsewhere in the book do not confirm the extent of the rise shown.

The author's estimates of total construction are critically compared with two similar estimates, one by Corrington Gill and one by Simon Kuznets. In addition, much of the information available on various aspects of public works activities are brought together and summarized. Unfortunately, most of the series are not carried beyond 1932, although the data on the P.W.A. extend into the early months of 1935.

The data presented on public works make it clear that the recent experience does not provide a test of the theory of public works as a stabilizing economic influence. To be sure, there was a definite lag of public construction on the downturn in 1929–31, but far from compensating for part of the decline in private construction during the depression, public works expenditures fell from a level exceeding 3½ billion annually (from 1927 to 1931), to a total about half that amount in 1933. Subsequent estimates[3] show an advance to a total exceeding 2½ billion in each of the years 1934 and 1935. In other words, the statistics show Federal expenditures expanding, especially in 1934 and 1935, but failing by a wide margin to restore the level of total public construction to the pre-depression volume.

Two other significant findings may be noted as emerging from the statistics presented. The most important single object of public construction expenditures during recent years has been the building and maintenance of roads, streets, and bridges. This group has constituted more than 50 per cent of the total of public construction in each year since 1927, and its proportionate importance increased to 65 per cent of the total in 1933. This important branch held up better than other public works, perhaps partly because the machinery for Federal assistance had been in operation for some time, and partly because the gasoline tax and license fees for automobiles yielded an income which shrank less than other public revenues.

The second finding which is clearly demonstrated by the statistics appears in the section on the financing of public works. The reader is left with little doubt that municipalities (where public works expenditures were first to decline) encountered grave difficulties in providing funds for continuing public expenditures during the depression. Any policy of planned public works that is dependent to any considerable extent upon cities and counties must cope with the problem arising from legal and economic limits of the borrowing power of small governmental units during depressions. Cities and counties, which had contributed around 75 per cent of all public works between 1925 and 1929, accounted for less than 40 per cent of the sharply reduced total of 1933.

A great deal of discussion but comparatively little statistical evidence is presented on the necessity for having a plan drawn up ahead of time if public works are to be effective depression stabilizers. A few cities are cited

[2] In fact an examination of the tables presented by the author on pages 168 and 209 would seem to indicate that about 60 per cent of the increase shown for the 14 sample cities occurred in New York City alone.

[3] See *Survey of Current Business*, April, 1936, p. 16.

as having a commendable record, but this record is not used to provide the basis for a model plan nor for a comparison with some of the less favorable records. Perhaps it is the author's intention to emphasize the fact that, although absence of plans presents serious obstacles to the effective marshalling of public works to combat depression, still more important is the previous planning of finances to give some effect to the plans when the time comes to put them in operation. The author, however, is not particularly hopeful on the subject of local finances. He claims that localities cannot cope individually with such a national problem as business depressions and that in the future the chief financial reliance will continue to rest on the Federal Government.

The limitations imposed on the discussion of public works by the lack of reliable statistical data provide a challenge to workers in the field of construction statistics and of public finance. For instance, the mixture of data referring in one instance to permits, in another to contracts, and in still another to expenditures for force account work, requires nothing short of legerdemain to compound an estimate of actual construction activity. Data on employment are distinctly unsatisfactory, and construction costs information is so unreliable that the author refrains from undertaking an adjustment of the dollar figures he presents to account for price change. On the question of sources of funds, the available data are extremely disorganized. In the absence of any summary measures on local conditions, it was found necessary to treat the local government section as a series of case studies of sample cities.

The discussion of theoretical problems is largely non-statistical. The reader is left with the impression that the statistical facts fail to fit the theoretical questions propounded. The theoretical discussion summarizes most of the questions already so widely discussed by others in the abstract, but practically none of the problems—extent of primary and secondary employment, comparisons of experience where advance planning did and did not exist, or an appraisal of the effects of timing of public works—is illuminated by any statistical evidence presented.

A most serious shortcoming in the presentation of the information contained in the book is the almost complete lack of charts. There are only three graphs in the book, and none of these presents series of basic significance.

HENRY B. ARTHUR

Washington, D. C.

Standards of Living in the Cleveland Metropolitan District, Special 1935 Report of the Real Property Inventory of Metropolitan Cleveland, by Howard Whipple Green. 1935. 695 pp. $100.00.

In this volume the detailed tabulations of the 1934 Real Property Inventory of Cleveland are presented. Cleveland has the distinction of being the only city of the 64 covered by the Federal Real Property Inventory for

which tabulations were made by census tracts.[1] This report, therefore, adds a wealth of information about each of Cleveland's neighborhood units (census tracts) to the store already accumulated and published by the Real Property Inventory of Metropolitan Cleveland. Data are presented in sets of 7 tables for each of the census tracts, for Metropolitan Cleveland, for Cleveland proper, and for the metropolitan area outside Cleveland. These tables contain information on: (1) family units by tenure, occupancy, number of rooms, type, and monthly rentals; (2) family units by tenure, monthly rentals, type, density of occupancy, condition, and facilities available; (3) owned homes, by value and encumbrance; (4) residential structures by type, age, condition, and the number of garages and automobiles; (5) family units by type of heating apparatus, by fuel used, and whether running water was available; (6) time required and mode of transportation used by wage earners to get to work; and (7) apartments by monthly rental per room, by number of rooms, and by persons per room.

There are 42 outline maps of the metropolitan district on which are shown by means of dots the differences among census tracts with respect to condition, value, rentals, facilities, and other housing characteristics. These maps provide a detailed picture of the residential composition of the city and its environs. A fold-in street map of the district showing census tract boundaries is inserted in a pocket.

The five pages of text describe the tables and also indicate the use to which the data can be put. Several census tracts in Cleveland are contrasted, and Cleveland is compared with other cities for which information is available.

Numbering of the maps and the provision of an index to the tables and maps would greatly facilitate use of the volume. The numbers indicating the census tract on each page do not stand out from the page. The use of larger figures would seem desirable. In other respects, the presentation is admirable. It is to be regretted, however, that a breakdown was not made of the large rent groups, $20–$29 and $30–$49. The ranges in these significant groups are much too wide.

As Dr. Green points out, the conditions revealed by the Real Property Inventory are not such as to make native Clevelanders point with pride, and little solace can be obtained by comparison with worse conditions in other cities. The Real Property Inventory data provide valuable ammunition for those who are trying to improve housing conditions and living standards. Also, they assist, when tabulated by small areas, in the solution of the many problems involved in large-scale housing, such as the selection of a site, the determination of the group to be accommodated, the rents to be charged, and other problems.

THEODORE A. VEENSTRA

University of Pittsburgh

[1] Other cities, notably New York, Philadelphia, and Pittsburgh, which conducted real property inventories as local projects tabulated the results by census tracts

Maritime Trade of Western United States, by Eliot Grinnell Mears. Stanford University: Stanford University Press. 1935. 538 pp. $4.00.

Since the dominant purpose of this pioneer study of a most interesting phase of the frontier of occidental civilization is to discover and interpret significant trends, the author does not pretend to present the subject of water-borne trade in other than broad outline. His chief tools of investigation are statistics, geography, and history. Especially worthy of mention is the fact that Professor Mears constantly keeps the world picture in mind. He finds that although one may speculate concerning the possible successful conclusion of gatherings of representative men of affairs and trained students who are both able and willing to think and act internationally, there is definite merit in such meetings, "if for no other reason than to clarify divergent issues" (page 371).

Chief attention is directed to foreign commerce and to the protected intercoastal and non-contiguous trades. Although coastwise traffic along the Pacific between United States ports is large and has internal significance, it is of less economic importance to the region than its intercourse with foreign nations, with the Atlantic seaboard, and with the territories of Alaska and Hawaii.

Western United States, "the most progressive section of the nation" (page 39), is larger than the Pacific Coast states of California, Oregon, and Washington. This region embraces, "as a tributary area," the Rocky Mountain states of Montana, Idaho, Wyoming, Utah, Nevada, Colorado, New Mexico, and Arizona. The non-contiguous territories of Hawaii and Alaska are also included. Guam, the Philippines, and American Samoa are excluded because of their greater alignment with the Orient than with the Western Hemisphere. Maritime commerce of the West Coast has been stimulated by the growth of population but even more by the increase in the number of new trade routes made possible by the opening of the Panama Canal.

Professor Mears calls attention to the "superabundance of Coast optimism relative to the great future of the Pacific area" and warns of the "manifest dearth of bona fide concrete information concerning it" (page 432). Nevertheless, after painstaking efforts to synthesize a great variety of interrelated factors involved in an evaluation of a perplexing problem, he calls himself "a cautious optimist," at least for the future of our Western States. He finds that the center of the world's commerce is definitely shifting to the Pacific with accelerating speed, although he admits that European trade still dominates United States foreign trade today. The merchandise imports of Western United States "increased from $60,000,000 in 1900 to $129,000,000 in 1913, to $523,000,000 in 1929, and then declined to $121,000,000 in 1933; correspondingly, merchandise exports gained from $72,000,000 in 1900 to $147,000,000 in 1913, to $595,000,000 in 1929, and declined to $198,000,000 in 1933" (page 446). The recent "abnormal slump" in trade is a "temporary phenomenon."

In his dual rôle of historical geographer and economist, Professor Mears

considers such subjects as coast ports and services, freight and passenger traffic, shipping conferences, and subsidies. There are several chapters devoted to geographical and historical background. Chapter VII—Some Pitfalls in Shipping Statistics, and Appendix D—Official Statistical Sources, warn the reader that a large amount of the data concerning the region is published by biased institutions, which issue reports without accompanying explanations of source or method. It is necessary to recognize the distinction between transportation statistics, which show place-to-place movement, and trade statistics, which indicate the commercial exchange of goods. According to the author, "trade statistics may show either the immediate exchange between regions in which are located the seller and the buyer, or they may show the movement between the regions of origin and final destination" (page 148). Although sometimes identical with *transportation* statistics, and again with *ultimate trade* statistics, immediate trade statistics often are not identical with either.

It is unfortunate that separate organizations publish figures which "differ hopelessly," though they presume to cover the same commodities. There is a distinct lack of agreement with respect to (a) the period involved, (b) the classification of sea "trades," (c) the classification of commodities, and (d) the units. Despite these limitations, the different records constitute "the one indispensable basis for observing the movements in merchant shipping" (page 418).

Those whose limited time prevents a reading of the entire volume and a study of its numerous charts, graphs, and tables of statistical data will enjoy perusing Professor Mears' Résumé and his Forecast. It may also prove interesting, some twenty years hence, to pick up this book again and to observe to what extent the interpretations have been borne out by history.

WILLIAM H. KOENIG

Columbia University

Earnings of Skilled Workers in a Manufacturing Enterprise 1878–1930, by Evan Benner Alderfer. Philadelphia: University of Pennsylvania Press. 1935. xi, 88 pp. $1.50.

"The most startling discovery" of this monograph, submitted as a doctoral dissertation at the University of Pennsylvania, is that, "at the early age of 31 and thereafter, earning power should be on the decline and that better than average earning capacity should be confined within the narrow range of 24 to 38 years of age. . . . Our results serve at least to raise the question whether this attitude," expressed as the crushing effects of mechanization "is not unduly tinged by overzealous veneration of the past." In these words, Dr. Alderfer summarizes and gives emphasis to his analytical survey of data (made available by an anonymous firm) covering 52 years of annual earnings and occupational experiences of a group of 3,823 apprentices and of similar data covering 2,413 journeymen from 1895 to 1930.

Other facts providing some inductive confirmation of our theory relating to labor mobility, earnings, and the problem of unemployment are given. For example, voluntary separations increase during the up swing of the business cycle; dismissals and lay-off, particularly among the higher paid journeymen, were resorted to in depression years; 90 per cent of the apprentices withdrew from the company by the end of their third year of employment; of the journeymen entering the employment of the company at the age of 18, 61.5 per cent withdrew at or before the age of 31; money wages paid the journeymen in 1920 were 28 times the amount paid in 1895, although the value of output had increased in this period only 15 times; the curve of earning capacity of the workers closely approximates the normal curve, except for those years in which changes either in the methods of wage payment or in the arts occur, with a tendency towards greater dispersions in years of prosperity. In Chapter VI, the author analyzes the earnings and employment records of 49 workers employed by the company for 20 years or more. The average annual earnings and the regularity of employment were uniformly above those for any other group comparable with respect to age, experience, and training.

The effectiveness and value of Dr. Alderfer's work would have been enhanced if he had related his findings to what others have done in this field. Surely, to mention only two contributions in this field, the works of Professor W. I. King and Professor P. H. Douglas deserve consideration. Since no mention is made of union organization, strikes, and other problems in labor relations, are we to conclude that in 52 years this anonymous company experienced no difficulties of this sort? These and other questions lead the reviewer to feel that the author has given too much attention to statistical techniques and not enough to the consideration and interpretation of important factors which lie outside but which nevertheless affect the data and conclusions of the study.

JACOB J. BLAIR

University of Pittsburgh

Publications of the Statistical Institute for Economic Research, State University of Sofia, Bulgaria. No. 1. 1935.

This is the first issue of the publications of the Statistical Institute for Economic Research, organized in 1935 (by special law) at the State University of Sofia. The Institute is financed partly by the Rockefeller Foundation and partly by subventions from the national and the agricultural banks of Bulgaria. The purpose of the Institute is an impartial study of the problems of the national economy of Bulgaria, particularly its current business situation, as well as of the world economic problems affecting Bulgarian national economy. It is a new Institute which will pay much attention to the analysis of the business conjuncture. The organization of such an institute in one of the Balkan agricultural countries must be greeted. These

countries have their particular economic problems, their business much depends upon their agriculture, and, for this reason, the studies of the Institute may be of particular interest.

Among the articles published in the first issue must be mentioned a paper by Professor O. Morgenstern, associated with a similar institute in Austria, discussing the organization and the problems of a conjuncture research, and an article by Professor Oskar N. Anderson, studying the price disparity problem in post-War Bulgaria. The fact that Professor O. Anderson, a well-known statistician, one of the initiators of the "variate difference method" in statistical theory, has been appointed the Director of the Bulgarian Institute may be a guaranty that its publications will be on the level of scientific standards.

All studies are published not only in Bulgarian but also in one of the West-European languages: English, German, or French, which makes them accessible to the readers outside of Bulgaria.

V. P. TIMOSHENKO

U. S. Department of Agriculture

Industrial Organization in India, by P. S. Lokanathan, No. 4 of the Studies in Economics and Commerce of the London School of Economics and Political Science, edited by Professors A. Plant, L. Robbins, and A. J. Sargent. London: George Allen and Unwin Ltd. 1935. 413 pp. 15 shillings net.

Dr. Lokanathan's *Industrial Organization in India* is an excellent and scholarly work in a field that has often been clouded by partisan discussion. It is an analysis of the financial organization of Indian industry by an Indian who is thoroughly conversant with the facts and the problems and who is not seeking either to place blame or to glorify. The preparation of the book has taken five years of study during which the author not only visited the important industrial centers of India but also traveled to Europe to gather comparative material. The result of this labor is a well balanced treatise.

The story of the organization of Indian industry is primarily a story of the managing agency system which is so distinctively a feature of Indian large-scale industry. The system was first imported by the British as a convenient means of providing both capital and trained leadership in a country where indigenous capital was scarce and administrative and technical skill had to be imported. It was the basis upon which the early jute mills, the tea estates, and the coal mines were first developed. The managing agents provided the first capital and acted as the managers of the enterprise they promoted. The system was perhaps the most effective one that could have developed under the existing conditions in India at that time. As Dr. Lokanathan says:

"But for the managing agency system the pace of industrial development

in India would have been slower, and the opportunities for British capital and British enterprise to function in India would have been limited. Thus England and India both gained."

Indian capitalists, especially in the cotton industry in Bombay and Ahmedabad, took over the system from the British, though with certain characteristic differences. The British managing agency, at its best, combined financial administration with industrial experience and knowledge of the technical problems of the industry involved. The Indian managing agency firms have been financial rather than industrial in character; and because of that fact they have often failed where the British have succeeded. Both types gave rise to abuse, especially in the practices through which managing agencies received remuneration; for a managing agency may receive not only the returns on capital invested, office expenses and a minimum commission for management which must be paid regardless of the success of the mill, but also commissions on production, commissions on sales and on purchases, and commissions on profits. Under this system, it is quite possible for a managing agent to be making money on a mill that is actually losing money.

The concentration of control in the hands of the managing agents rather than in a board of directors; the speculative sale of the managing agency for purely financial reasons and without consideration of the industry; the provision of credit on the good name of the agency rather than on the industrial project; and questions of efficient management are some of the problems that are analyzed and discussed in this volume. There is also a very good chapter on the localization of industry and a last chapter on wages, standard of living, and efficiency which summarizes the fundamental aspects of the labor factor in Indian industry with interesting comparisons with Japanese and Western labor.

JOHN E. ORCHARD

Columbia University

Die Groszhandelspreise in Deutschland von 1792 bis 1934, by Alfred Jacobs and Hans Richter.

Sonderhefte des Instituts für Konjunkturforschung, Nr. 37, Berlin: Hanseatische Verlagsanstalt, Hamburg. 1935. 111 pp. 15 RM.

This study presents an index number of wholesale prices in Germany covering about one and a half centuries. There are 224 series of prices of 45 commodities for 106 markets. The index numbers are a weighted arithmetic mean with 1913 as the base year. The authors point out that for short periods of time, index numbers based on stable weights may be satisfactory because the composition of trade does not change decidedly. But if the index numbers cover a long period, changing weights are necessary. This is especially important in this study of prices in Germany because of the shift from a predominantly agricultural economy to an industrial one. For this reason the index numbers were calculated by three methods:

1. On the basis of fixed weights based on the average consumption per capita in the years 1850 to 1913. The proportion of agricultural foods to industrial products was 1:1.

2. The 143 years were divided into 5 parts, with fixed weights for each period, but differing from period to period. The periods were as follows:

1792 to 1830—The Napoleonic War period.
1820 to 1850—Relatively stable prices.
1840 to 1873—Upward movement of prices.
1860 to 1895—Downward movement.
1885 to 1934—Generally rising prices.

In each case index numbers were calculated for an overlapping period of 10 years. The index number for each period was weighted by the average consumption of the period. The weights for home grown agricultural products varied from 73 per cent in the first period to 30 per cent in the fifth.

3. An index number was calculated with yearly changing weights by the chain method.

The price series were generally comparable with respect to kind, quality, measure, point of sale, and usage. When a change in grade occurred, an overlap of three to ten years was used to test the degree of correspondence of the movement of the prices. Differences in weights and measures were eliminated by conversion to kilograms and liters.

Because of the lack of uniformity of the German currencies before 1873, all prices were computed in terms of the goldmark. The silver prices were converted to gold at the ratio 1:15.5, which was the rate at which the change to the gold standard was made in Germany in 1873. The authors did not consider the year to year fluctuations in the ratio. This seems to be justified by the fact that the changes were small; and the average of all years was 1:15.6.

The study includes a short summary of the history of the most important German currencies: Hamburg Mark Banko, Prussian Taler, and South German Gulden.

Another section of the study deals with the major fluctuations of prices. The authors found 26 cases when prices changed more than 10 per cent in one year; and 12 cases when the change was more than 25 per cent in a period of seven years or less.

The study clearly indicates that, although year-to-year fluctuations in prices in Germany departed from those prevailing in the United States and England, the long-term trends are very similar. Furthermore, the index numbers of individual commodities and groups moved much the same as those in other countries. The authors found that chemicals, textiles, and colonial products fell relative to the general price level; and that agricultural products were stable or rose. These long-time relationships are in apparent agreement with those found in the United States.

The student of prices will appreciate the detailed tables of commodity

prices with copious footnotes in the appendix and the comparative index numbers based on fixed and variable weights.

The student of money and history will appreciate the careful summary of German monetary history in the 19th century. The casual reader can summarize quickly the major changes in the prices of individual commodities as well as the general price level by studying the 29 well-constructed charts.

This study is a thoroughgoing analysis that every student of prices, money, and economic history will find worth while.

BARBARA GOK

Ithaca, New York

American Bank Failures, by C. D. Bremer. New York: Columbia University Press. 1935. 144 pp. $2.25.

The dismal side of banking in the United States has not received consideration commensurate with its importance. Although insolvency has constituted the primary disturbance in the post-War banking revolution, the literature of banking has relegated the subject to a minor position. This work on American bank failures is a creditable attempt to overcome the deficiency.

The book begins with a general summary of bank failures since 1865, special emphasis being given to the debacle of the 1930's. Succeeding chapters cover (1) failures before 1921, (2) failures, 1921–1933, (3) liquidation of national banks, (4) liquidation of state banks, (5) responsibility for failures, (6) banks and the security markets, and (7) the guaranty of deposits. Most unfortunately the book went to press shortly before the enactment of the Banking Act of 1935. Discussion of current legislation on the guaranty of bank deposits is based on the banking bill passed by the House of Representatives and on the recommendations of the Senate subcommittee. Significant alterations were made in the bill as it was finally approved on August 23. It would be desirable to have a leaflet inserted indicating the changes, although the reader can get all the required information by referring to Title I of the Banking Act of 1935 (reprinted in the *Federal Reserve Bulletin*, September, 1935).

As a non-technical discussion of mortality rates in banking since 1865, *American Bank Failures* is a valuable addition to banking literature. It covers most of the outstanding phases of the problem. Contentions are usually supported with factual material. The main "field work" was done in the collection of data on liquidation of state banks, a previously neglected angle of the subject. Data obtained on these liquidations were admittedly cursory. They reflect the perfunctory nature of control exercised by state banking departments. The chapter on the liquidation of state banks accordingly is as suggestive for what it necessarily omits as for what its includes.

Many readers will be interested in the examples (Chapter 6) of concentrated ownership in small unit banks. Local banks too frequently are not community projects—as they are supposed to be. Typically, they are instruments of small cliques. The author does not investigate ownership of

chain or group banks, nor does he consider any possibility of getting better representation of the public in the banks. The argument, therefore, is not decisive one way or another, although the author manifests opposition to the small unit banks.

The book has limited usefulness for those desiring a thorough discussion of alternative choices available for elimination of bank failures. Statistical material is selective, and it is subjected only to elementary analysis. The first three chapters are marred by occasional loose description. The statement is made, for example, that 19,000 banks were "placed in receivership or forced to restrict or suspend operations between 1865 and December 30, 1933, on account of inability to meet the demands of depositors" (p. 12). The qualification at the end of the sentence is misleading in that the figure quoted refers to total failures including those not associated with depositors' demands.

Some phases of bank failures that a work of this general scope should include are neglected. Nothing is mentioned about seasonal tendencies in failures, although the seasonal factor probably had a considerable influence on some of the data (e.g., those given on p. 14). One would like to know, for defunct national banks at least, the variability of payments to depositors and the duration of the receiverships. Could depositors count on a reasonably large percentage of recovery within a reasonably short time, or were they subjected to great uncertainty and endless delay? Surely these points should not be overlooked even in a book dealing mainly with summations and central tendencies.

WILBERT G. FRITZ

University of Pittsburgh

Recording of Local Health Work, by W. F. Walker and Carolina R. Randolph (in cooperation with the Committee on Administrative Practice of the American Public Health Association). New York: The Commonwealth Fund. 1935. xvii, 275 pp. $2.00.

The foundation of all sound statistical work is the accumulation of reliable basic data. Therefore this handsome volume is of significance for all statisticians although its more direct usefulness will be confined to those members of the fraternity who are working in the field of health administration.

The need for better records of health service has been generally realized as a result of the work of the Committee on Administrative Practice of the American Public Health Association during the past fifteen years. Efforts to meet that need have been furthered by the Commonwealth Fund in connection with its health demonstrations, by the State Department of Health of Tennessee under the leadership of Dr. E. L. Bishop, and, particularly, by the Sub-Committee on Record Forms of the Committee on Administrative Practice under the chairmanship of Dr. G. C. Ruhland.

The present volume is based on and embodies all this earlier work and presents, in full size facsimile, report forms and record forms useful in health

department budgeting and accounting, in the registration of vital statistics, individual and family folder records, medical and nursing records, and clinic records, placards, quarantine notices, complaint forms, records of services and case histories useful in communicable disease control, maternity and infant welfare and school health service, dental services, sanitary inspection, food and milk control, and laboratory service. The purpose of each form and its method of use are explained and brief chapters at the beginning and end of the book discuss the philosophy of record-keeping and the interpretation and graphic presentation of data.

The forms presented are the result of years of experimental use, and they bear the approval not only of the American Public Health Association but also of the National Organization for Public Health Nursing, the American Social Hygiene Association, and the National Tuberculosis Association. In preparing and publishing this volume, Dr. Walker and Miss Randolph and the Commonwealth Fund have rendered an invaluable service to the cause of sound public health practice.

C.-E. A. WINSLOW

School of Medicine
Yale University

Les Migrations de la Population Intéressant le Territoire de la Tchécoslovaquie Actuelle (depuis le XVII• siècle à nos jours), by Ivo Sasek. Geneva. 1935. 174 pages. 5 s.f.

The task of describing the migratory movements across the border of present-day Czechoslovakia during the last three centuries is as interesting as it is difficult, owing to the fundamentally different political history and administration of the parts comprised in the state created in 1919. The author of the present study has the merit of having compiled a great amount of data referring to the problem from numerous sources, although he did not quite succeed in molding the multiform data won from statistics of widely differing character into a concentrated and lucid presentation. This criticism refers particularly to the parts concerned with the more recent migrations.

The most compact and illuminating chapter refers to the older migrations during the 17th and 18th centuries, which were in the main caused by religious persecution of the Bohemian protestants through the Austrian counter-reformation up to the reign of tolerant Emperor Joseph II. The story of these emigrants, the "flower of the nation," their taking refuge in the neighboring countries in constant hope of armed intervention from outside powers, and their ultimate disappointment bears features of parallelism to similar phenomena of our day.

During the 19th century and up to the World War, emigration was caused by economic reasons: particularly from the poorer and more backward agricultural areas, such as Southern Bohemia and Slovakia, which sent high percentages of their large birth surplus across the borders. From 1901 to 1910, it is estimated that the territories of the present state lost by migra-

tion as many as 50,000 persons per year, i.e., one-third of their natural increase.

The new republic has made great efforts to provide outlets for its partially still rapidly growing population. In particular, the Agrarian Reform, which abolished the latifundia, operated in this direction. Economic opportunities, however, could and still cannot be developed as rapidly as the growth of the population would require, so that the government has mainly concentrated on regulating emigration and impeding exploitation of and discrimination against its emigrants. The author approves of this policy. In view of the restrictions imposed on immigration in the United States and some other countries, he is especially in favor of leading the stream increasingly into France, where Czechoslovakian immigrants adjust themselves easily to climatic and psychological conditions. Interesting with respect to similar problems in other countries is the suggestion that France should open one of her colonial possessions in North Africa to peaceful penetration by Czechoslovakian agriculturists. Still, he believes that development of domestic resources and regulation of emigration will have to be supplemented by encouragement of birth control in the most backward parts of the state.

An extensive chapter on the development of Czechoslovakian immigration into the United States will be useful to American students of immigration problems because of the compiled data on the composition and geographic distribution of this group of immigrants.

It is regrettable that the otherwise useful study does not contain any maps, which would greatly facilitate its reading.

Marie Jasny-Philippi

Washington, D. C.

A Test of the Consumer Jury Method of Ranking Advertisements, by Neil H. Borden and Osgood S. Lovekin. Business Research Studies—No. 11, Harvard Business School. 1935. v, 61 pp. $1.00.

This study of the Consumer Jury method of ranking advertisements is an important contribution toward the ultimate solution of a controversy which has long been vexed by acrimony. Borden and Lovekin have secured a number of series of advertisements the relative effectiveness of which was known through records of direct sales or of inquiries. They submitted these advertisements to various consumer juries to determine the relationship between the preferences of these juries and the known *relative* effectiveness of the several advertisements in each series. They conclude, in general, that this method of ranking advertisements is valid in judging differences in physical layout of advertisements, but that the necessity of using a jury spontaneously interested in the product or service advertised for rating advertising appeals restricts the applicability of the method, ordinarily, to products having fairly general use or interest.

When a study is so patently careful and significant, a consideration of possible limitations in its application to actual advertising cannot be regarded as a derogation.

The first question that the analysis suggests to the reviewer is whether any ranking of appeals can be trustworthy unless the jury used is typical of the audience supplied by the particular advertising medium expected to carry the advertisement. It is clear that appeals which would be dynamic for readers of the *New York Times* might not be dynamic for readers of a sensational tabloid, and *vice versa*. More important is the possibility that the editorial content and associative *milieu* of a magazine will condition its readers for or against a particular appeal, so that the same reader might be responsive to an advertisement in one magazine, and not to the same advertisement in another magazine. There is considerable reason for believing that it is more important to test the responsiveness of particular audiences than to rate several appeals by means of an unconditioned audience.

A similar consideration—especially in view of the authors' requirement of a jury spontaneously interested in the product or service advertised—is the doubt whether this technique is applicable to those appeals stimulating primary demand as well as to those stimulating selective demand. Since the stimulation of primary demand requires the arousing of a latent want, it might be difficult to select a jury suitable to the task.

Finally, there seems to the reviewer a real doubt concerning the adequacy of the statistical approach recommended to determine the number of interviews needed for reliability. In dealing with an adequate number of random occurrences from a reasonably homogeneous whole, obviously, the theory of probability is validly applicable, and the formula for P.E. can be applied. But the considerations adduced so far all point to the fact that the whole is not at all homogeneous, nor the instances constituting any sample adequately random. It therefore seems to the reviewer that the statistical approach may be somewhat too rigorous, and that more consideration should be given to the number of variables which are likely to operate against homogeneity and random selection.

In view of the importance of the problem which the study analyzes, and in view of the difficulty of controlling the variables, it seems most desirable that the authors make other analyses of the consumer jury technique, so that users of the method will have more guidance, and so that the method can be appraised on the basis of the analysis of a greater number of instances, and with a greater variety of application.

LAWRENCE C. LOCKLEY

Division of Commercial Research
The Curtis Publishing Company

Higher Control, by T. G. Rose. London: Sir Isaac Pitman & Sons, Ltd. 1935. xvi, 269 pp. $3.00.

This treatise is written with a British background and must be read with this in view if it is to be appreciated. The author offers an argument for presentation of the essential accounting figures in a less technical and more

usable form than is apparently the custom in Great Britain. He argues further for comparative statements and for the use of a twelve-month moving total compared with the results of the last fiscal period rather than comparisons of individual months in different years. "Higher Control" is defined as "a monthly survey of the functional activities of a commercial undertaking, carried out from the business, trading, and financial viewpoints, and based upon direct trend comparison between the position at the moment and the position at the last financial year."

Although great care is taken to point out the differences between higher control and budgetary control, considerable space is given to an illustration which could well have appeared in any text on budgets. A tabulation of the steps involved in "the installation of higher control" shows five steps: three in the "programme stage"; and one each in the "watching stage" and the "action stage." The steps proposed in the business or sales division show striking similarity to the steps involved in the installation of a budget.

Programme Stage	
First Step	Decide on desired profit, either: (a) On net worth; or (b) Reasonably attainable
Second Step	Fix turnover (sales volume) necessary to obtain desired profit
Third Step	Split turnover by products and territories
Watching Stage	Watch: Sales trends (viz.: upward or downward slope of monthly moving annual trend)
Action Stage	Take action: To increase sales

In the watching stage a departure from ordinary budgetary procedure may be noticed in that comparisons are with the monthly moving annual total rather than with the results of the Programme Stage. This shift apparently throws overboard many of the benefits obtainable from that stage. Budgetary authorities in this country would point out two types of advantages from the use of budgets: planning and control. Higher Control uses the first but then seeks its control in other directions.

The general thesis of the book is presented in the first five chapters. The balance is divided into two parts. Chapters 6 through 9 are a detailed discussion of the problem of presentation of results with individual consideration of each important item. Chapters are devoted to the business (sales) position, the trading (operating) position, the financial position, and control reports. A feature of real interest is the inclusion of a complete set of Board Reports for presentation to the board of directors. There is also a discussion of the more common financial ratios. The last three chapters are descriptions of particular applications of higher control to retail distribution and factory operations and a summary statement of the uses of higher control.

The reviewer is forced to the conclusion that there is little really new in

this book. To those American industrial accountants and business men who have been accustomed on the one hand to prepare and on the other hand to demand accounting reports designed for control uses, the discussion of higher control will appear at times naïve. Nevertheless, reading of what is an interesting presentation will remind even such persons of the distance we have traveled and perhaps give them new enthusiasm for further progress. It is needless to say that there are many in this country who need to read and reread a book of this type until they finally realize that accounting and statistics are service functions which are utterly futile unless the results can be and are used by those for whom the service is performed.

WYMAN P. FISKE

Massachusetts Institute of Technology

Administration of Public Employment Offices and Unemployment Insurance. Canada, by Industrial Relations Counselors, Inc.; *France*, by A. Gilbert; *Sweden*, by K. Bergstrom and Associates; *Switzerland*, by Dr. F. Mangold. New York: Industrial Relations Counselors, Inc. 1935. xi, 397 pp. $3.50.

This is the third volume in the Unemployment Office and Unemployment Insurance Administration Series, of the Industrial Relations Counselors, Inc., the two earlier volumes being devoted to Great Britain and Germany. This volume deals mainly with the placement work of the public employment offices in the four countries mentioned in the title but includes brief accounts of the unemployment insurance systems in all but Canada.

The treatment of these subjects is almost exclusively descriptive, with little attempt at appraisal. It is distinctly "an inside account," being based upon statutes, rules and regulations, official statistics, and information gained at headquarters. Nothing at all is given on the reactions of employers and employees toward the public employment offices. The only conclusions reached are that the Federal-provincial system of the organization of the Canadian employment offices, as it existed prior to the recent nationalization of this service, had "many limitations," and that voluntary, governmentally subsidized unemployment insurance in France has not provided protection to nearly as large a percentage of the workers as have the compulsory systems of other countries.

In the Foreword the hope is expressed that this series of research studies will have "practical value" for the "organizers and administrators" of the United States Employment Service, "as well as for business executives, labor officials, and others concerned with its social significance." The "practical" aspect seems doubtful, particularly for others than the "organizers and administrators," but the genuine "value" is very certain. This book makes available for the first time in English, a comprehensive, and no doubt accurate, account of the organization of the public employment offices in four countries and, in a briefer manner, gives a description of the unemployment

insurance systems in three of these countries. By itself, it may have only very limited "practical value," but as a source of information for a broader treatment of these subjects it is distinctly worth while.

EDWIN E. WITTE

University of Wisconsin

The Budget in Governments of Today, by A. E. Buck. New York: Macmillan Co. 1934. vi, 349 pp.

The reader who wishes a broad view of the budgetary systems of the chief nations of the world will find in this volume a simply written account that will introduce him to the subject and at the same time present him with some clearly defined conclusions that have been reached by the author in the course of his long experience in the study of budgets. The basic considerations that weigh most with the author in reaching his conclusions are usually given, but supporting data are not presented in abundance, and the volume seems indeed to be intended more for the general reader in government than for the research student of budgeting—with the important qualification that the many significant items respecting budgetary practice abroad will add to to the information of, and above all suggest new fields of research for, the American student. Probably the best feature of the book is the constant comparisons and contrasts of country with country that it lays before the reader.

The English system of budgeting is the one to which the author most inclines, on the whole. Thus, in suggesting changes in Federal budgetary procedure, he says that appropriations should be made in lump-sum amounts rather than in segregated items, and the administrative officers should not be permitted to spend these appropriations without the approval of the executive; the executive should explain his budgetary proposals before the legislature in open session, and the legislature should not attempt to produce through its committees a substitute plan of its own; the controlling and accounting functions should be separated from those of post-auditing, and a special Congressional committee should review the results of the post-audit.

The attempt of the book to cover a large area necessarily involves the author in some interesting problems of delimitation of his field, especially in Chapters II (General Aspects of the Budget) and V (The Budget as a Financial Plan). Federal control of state administrative standards with respect to highways, education, and health (p. 61), Federal-state-local tax relations (p. 62), expenditure retrenchment (p. 74), and, indeed, the general problem of whether a budget should be balanced during depression (pp. 70, 115) are touched upon. Unfortunately, the treatment given these topics is so brief and inadequate that it is in danger of leaving the general reader with a seriously over-simplified view of some important and complex problems. On the other hand, the author does not include within the scope of his

survey such important parts of budgetary technique as the relative accuracy with which different kinds of expenditure and tax yield can be estimated, and the methods available for estimating the yield of progressive-rate taxes. It would be helpful to get the opinion of budget students on the nature and extent of the difficulties of forecasting for so-called "cyclical" budgets, especially in view of the current tendency of emergency expenditures to become permanent expenditures when checks on expenditures are as few as they are under existing American practice.

A selected bibliography with comments and a highly detailed index add markedly to the usefulness of the volume.

CARL SHOUP

Columbia University

The Presidential Vote, 1896–1932, by Edgar Eugene Robinson. Palo Alto, California: Stanford University Press. 1934. ix, 403 pp. $6.00

Voting Behavior in the United States, A Statistical Study, by Charles Hickman Titus. Berkeley: University of California Press. 1935. 74 pp. $.75.

Both publications present the results of what purports to be the application of statistical methods in the field of political science, the first being primarily a collection of voting data for all counties in the United States, whereas, the second is concerned with a statistical analysis of voting data. Both are disappointing, the first because the historian failed to appreciate the difference between primary and secondary sources, and the second because visual or graphic approximations and very elementary methods were used to solve important and complicated problems.

Professor Robinson's book is principally a compilation of data, for out of the 403 pages 358 are given over entirely to tables and notes which explain them or list sources, and the remaining 45 pages include ten full-page plates showing for each of the ten presidential elections a map with Democratic counties in black, Republican counties in white, non-Democratic, non-Republican counties dotted, with other designations for tied counties, no returns, unorganized, and territories.

On page vi we read:

> For the use of students who wish to go more deeply into this study and this method, extensive notes have been added, both in explanation of the distribution of the vote and in comment upon the returns from each of the states. It is believed that the ground work has been laid for innumerable studies of economic and social, as well as of political, significance. Herewith are presented the vote and its distribution. ***This is the historical record.*** (Italics added.) Others will use it as their interests and methods dictate.

The author seems rather optimistic when he writes, "This is *the* historical record." Perhaps it is only another historical record, for the reviewer has found that there are too many historical records. Textbooks on American

government and history, almanacs, and official state documents differ seriously in many cases. Professor Robinson recognizes this difficulty on pages 131–132:

> Many state documents and a number of manuscript returns contain inaccuracies in addition and transposition of figures and names. It has been found that state totals published in state documents and in other printed sources are frequently changed in subsequent editions of the same kind, yet changes in county returns have not been made to accord with new state figures. Computations have been checked, obvious transpositions rearranged, and typographical errors corrected. Readers will find, therefore, that the totals by state and nation herein presented do not always correspond with state totals as reported in state documents or with national totals which, although appearing in national documents, are based upon state reports.

The prospective user of this book may well ask if the changes made by Professor Robinson were really beneficial. Did his change or failure to change always increase the accuracy? Only a careful study of the data can provide an answer, and the limitations of this review prevent an adequate discussion of the study the reviewer has made. For Missouri, 27 errors were found, two of them so serious as to give the victory to the wrong party (Caldwell County in 1900 and Pemiscot County in 1924). Twelve errors were found in the data for Illinois. Most of these errors occurred because Professor Robinson had too much confidence in other "accessible summaries, notably *The World Almanac*" when he found that the state votes did not agree with the totals of the county votes given in state manuals or documents. This procedure seems to ignore the important distinction between primary and secondary sources.

Professor Titus seeks to determine "scientific laws of voting behavior." The one law in which he has the most confidence at the end of his study is "The larger the P[population] or VP[voting population] of a political unit the smaller the VC[vote cast] relative to P[population] or VP[voting population]." This statement sounds as if it were the result of a correlation or covariance study, but the author did not use either of these techniques to determine the "law." Instead of correlating population and votes cast per 1000 population for one election or using covariance when seven or more elections were included, Professor Titus classifies the data with respect to size of population and computed averages of the votes cast per 1000 population for each election separately. After inspecting these results he formulates the "law." To some people such a general non-quantitative "law" could not be scientific, and moreover this law is not satisfactorily proved statistically. Professor Titus starts out with five classes of cities according to size. The mean number of votes cast per 1000 population does not increase regularly from the largest cities to the smallest cities, so he combines the four smallest classes of cities into two classes, leaving the largest cities as before. With these three classes he is able to show some negative relationship, but the scientific law is based upon only three observations—averages for large, medium, and small cities. This base is a weak foundation. Without

using the analysis of variance and covariance, he is at a loss as to how to combine his information for different elections. Presumably he does this mentally. No information is given on the number of cities studied or on the number in each class. A footnote referred to an article in the *Southwestern Political and Social Science Quarterly*, which contains Professor Titus' earlier work on the same data in 1928. Only 22 cities were studied, and only two of them fell in the class of large cities until late in the period when a third city became large enough to fall in this class. The large cities cause a very skewed distribution, and when they are eliminated the correlation is not significant. Furthermore, Professor Titus inspects correlation between means of classes, whereas he formulates his law in terms of correlation between original observations. These two should not be confused.

The work contains other illustrations of a poor choice of technique, such as the feeble attempt to separate the long time trend in the exercise of the voting privilege and the effect of extending this privilege to women.

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